NOTE TO USERS

This reproduction is the best copy available.

UMI®

University of Alberta

A Critical Analysis of the Paradoxical Nature of the Discourses of Ecology (1913-2000) and Outdoor Recreation (1960-2008)

by

Sean Edward Ryan ©

A thesis submitted to the Faculty of Graduate Studies and Research
in partial fulfillment of the requirements for the degree of

Doctor of Philosophy

Faculty of Physical Education & Recreation
Department of History & Classics

Edmonton, Alberta
Fall 2008

Library and
Archives Canada

Published Heritage
Branch

395 Wellington Street
Ottawa ON K1A 0N4
Canada

Bibliothèque et
Archives Canada

Direction du
Patrimoine de l'édition

395, rue Wellington
Ottawa ON K1A 0N4
Canada

Your file Votre référence
ISBN: 978-0-494-46416-8
Our file Notre référence
ISBN: 978-0-494-46416-8

NOTICE:
The author has granted a non-exclusive license allowing Library and Archives Canada to reproduce, publish, archive, preserve, conserve, communicate to the public by telecommunication or on the Internet, loan, distribute and sell theses worldwide, for commercial or non-commercial purposes, in microform, paper, electronic and/or any other formats.

The author retains copyright ownership and moral rights in this thesis. Neither the thesis nor substantial extracts from it may be printed or otherwise reproduced without the author's permission.

AVIS:
L'auteur a accordé une licence non exclusive permettant à la Bibliothèque et Archives Canada de reproduire, publier, archiver, sauvegarder, conserver, transmettre au public par télécommunication ou par l'Internet, prêter, distribuer et vendre des thèses partout dans le monde, à des fins commerciales ou autres, sur support microforme, papier, électronique et/ou autres formats.

L'auteur conserve la propriété du droit d'auteur et des droits moraux qui protège cette thèse. Ni la thèse ni des extraits substantiels de celle-ci ne doivent être imprimés ou autrement reproduits sans son autorisation.

In compliance with the Canadian Privacy Act some supporting forms may have been removed from this thesis.

While these forms may be included in the document page count, their removal does not represent any loss of content from the thesis.

Conformément à la loi canadienne sur la protection de la vie privée, quelques formulaires secondaires ont été enlevés de cette thèse.

Bien que ces formulaires aient inclus dans la pagination, il n'y aura aucun contenu manquant.

Canada

Abstract

This dissertation takes a critical look at how the discourses of ecology and outdoor recreation produce, arrange, and discipline knowledge and people. I follow select developments in ecology from 1913, when the first journal was published, up to the end of the century. Outdoor recreation (OR) has a shorter history and I focus only on the development of minimum impact policies and management techniques that began in the 1960s. I examine the consequences, benefits, and problems in way these discourses produce and organize knowledge and people. I employ Foucault's archaeology and genealogy to better understand the structure of both ecology and OR that I then subject to a poststructural analysis. Both fields are profoundly paradoxical. OR, for instance, emphasizes freedom from unnecessary social rules. At the same time, however, it authorizes and produces an astonishing level of surveillance as it disciplines users to comply with minimum impact protocol. An analysis of these fields in terms of their disciplinary regime and disciplinary techniques shows them to be grounded on a problematic division of humans from nature and then a subsequent rejection of the human as inherently damaging. By using the trace, différance, and hauntology, I show how elements that were divided and rejected, such as all signs of human presence, return in paradoxical, problematic, and shifting ways. Both disciplines employ various tactics related to this returning and certain consequences arise from these once-thought-to-be-excluded elements. Textual practices, such as the common but inconsistent use of single quotation marks around concepts that require or demand the absence of all human presence (for example, 'pristine,' and 'natural ecosystem'), are an example of the consequences that occur in both disciplines as scientists and researchers attempt to define

and explain their field of study. As a response to the ghosts and traces of the human presence, I make suggestions for a process for producing knowledge in ecology and OR that is informed by science studies' reconfiguration of agency to include nonhuman actants and my own understanding of the power and place of paradoxes in science. This process assumes a vastly different relationship between humans and nonhumans, one that would not require the same level of type of disciplining seen in ecology and OR.

Table of Contents

Chapter 1: Introduction--1

    The Nature of Theory: A Theory of Nature--6

    Archaeology and Genealogy--9

    Poststructural Agency---13

        Acting Like A Turnip---16

    Writing a Rotated History--20

        The Issue of Language: The Politics of Writing (and) History----------22

    A Naïve Hypothesis--32

    Chapter Outline---40

Chapter 2: The Structure of Science and Ecology--45

    Games of Truth, Regimes of Discipline---48

    The Disciplinary Structure of Ecology--55

        Progress, Development, and Stability ---------------------------------------67

        Mechanisms of Universalization and Standardization-------------------82

        A Disciplined View: Deep Ecological Vision--------------------------------94

Chapter 3: The Structure of Outdoor Recreation--109

    The Disciplinary Structure of Outdoor Recreation-----------------------------121

        You Can't Have Your Cake and Eat It Too: Freedom and Regulation in Wilderness---128

        Change and Authenticity--145

        Individuals in the Woods--157

Chapter 4: Humans and Nature---166

Modernity and the Enlightenment------170

The Human/Nature Split in Ecology and Outdoor Recreation:

    Where Did Everybody Go?------177

It's All About Nature?: Understanding Shifty Nature------179

Scary 'Nature': The Use of Scare Quotes------190

Paradoxes------199

Chapter 5: Tracing and Haunting Humans and/in Nature------211

The Necessary Failures of Normative Ecology------213

What is the Trace?------227

Ghosts in/of Nature------237

Chapter 6: The Nature of Paradoxes/The Natural Paradox------248

Theory of Paradoxes------249

    (Post)structural Paradoxes------253

Structure as Object: The Paradox of Structure and Structuralism------256

Science Studies------267

Chapter 7: Paradoxical Ecology and Outdoor Recreation------284

The Limits of Ecology and Outdoor Recreation------288

    Ecology in Outdoor Recreation------297

New Ecology: Chaos, Chance, and Complexity------304

    Chaos Models------307

    Landscape Ecology: Patch-Dynamics Models------315

Expanded Modelling: Human Thought In/And Nature------324

Paradoxical Ecology and Outdoor Recreation------328

Chapter 8: Conclusion--337

LIST OF FIGURES

Figure 2.1: Universal energy model--86

Figure 8.1: Archaeology of ecological discourse in terms of agency-------------------396

Figure 8.2: Archaeology of ecological discourse in terms of the nonhuman---------397

CHAPTER 1: INTRODUCTION

Every word is a preconceived judgment.
~ Friedrich Nietzsche, 'Human All-Too-Human'

The focus of this work addresses environmental issues that cross many divides. Since the 1970s, environmental concerns have been gaining ground in Canada both in terms of their profile and seriousness.[1] Over the same period, the use of wilderness and other protected areas has increased dramatically. As wilderness recreation became more popular, it should not be surprising to find a growing concern for the quality of the environment in the field of outdoor recreation (OR). One of the manifestations of this concern appears in the form of minimum or low impact camping practices. These practices began in the early 1970s, became more standardized in the early 1980s, and are now part of the basic discourse of outdoor recreation in terms of articles, brochures, educational and training programs, research studies, and common or best practices for outdoor users. These practices are designed to limit or reduce the negative effects that human users have on wilderness areas. While it is true that backcountry users are not forced to adopt these practices, minimum impact protocols are now widely disseminated throughout the OR field. In addition to minimum impact practices, OR also manifests its concern for the environment in debates and discussions about identifying and reaching proper management goals for wilderness areas. In both these trends, OR discourse regularly employs ecology to justify and legitimate its positions and conclusions. Comments that OR practices and policies are designed to improve or protect the ecology of a natural area are common in the discourse. Likewise, management goals and objectives often focus on the preservation or improvement of the ecological integrity of wilderness areas. While the link between ecology and OR is made explicit in the discourse, I am interested in examining the exact nature of this relationship and whether it might be restructured to increase the potential for protecting and preserving wilderness.

---

[1] For instance, Boyd (2003) remarked that 98% of Canadians view nature in all its variety as essential to human survival, 90% consider time spent in nature as a child very important, 85% regularly participate in nature-based activities, and 82% say nature has valuable spiritual qualities for them personally, 4.

I take as the temporal frame of this study the appearance, in 1913, of the first professional ecology journal, and follow ecological discourse up to the end of the millennium. In terms of outdoor recreation, there were a number of precursors to OR throughout the 1900s; however, the field did not become professionalized until the 1960s, at which point researchers began investigating the trends, patterns, habits, and benefits of using wilderness areas. University programs in outdoor recreation developed at this time, certification schools (for example, Outward Bound and the National Outdoor Leadership School) came to Canada, conferences and workshops were held across the continent, and articles began appearing in journals devoted to recreation and leisure studies (that themselves appeared in the 1960s). At the same time as OR matured into a profession, concern for the quality and future of the resource it depended on was being expressed. For example, it was at this time that studies of recreational impacts began appearing.[2]

In outdoor recreation, the environment usually refers more specifically to wilderness areas. Quintessentially, these are large and relatively undisturbed areas that contain only a certain amount and type of development. Of course, the particulars vary with location (ocean environments are quite different from arctic ones), with the level of protection (some areas forbid motorized vehicles while others allow them), and with the agency responsible for managing and maintaining the integrity of the area.[3] Nonetheless, wilderness areas can be considered as a specific type of landscape characterized by important similarities. One such similarity is an emphasis on naturalness. Wilderness areas are most often thought of as a type of natural area. Not all natural areas are wilderness, however. For instance, a small plot of abandoned rural land might be seen as natural, but would likely not be considered wilderness. For the purposes of this work, I engage the position that wilderness is a natural area large enough to allow ecosystem functions (for example, predator-prey relationships and succession) to operate and that has limitations on the amount and type of development and use.

---

[2] See Marsh (1970), Shafer, Hamilton Jr., and Schmidt (1969), and Willard and Marr (1970) for examples of early OR studies.

The natural sciences play a significant role in the conceptualization of what a natural area should look like and thereby also frame environmental problems related to wilderness and solutions to those problems. In particular, ecology, as the science that examines the interactions between organisms and their surroundings, could form a considerable part of the conceptual and methodological foundation upon which low impact practices and management goals are built and justified. The transition of knowledge from ecology to outdoor recreation, however, was not a simple or straightforward process. At the same time as models and theories were adopted, they were also adapted. Some of these changes are obvious and necessary, some are not. By initiating a conversation between ecology and outdoor recreation, I want to examine these adaptations and consistencies. To say that research in OR either bears no resemblance to or is identical with ecological studies of natural areas is simplistic. The contributions and influences of one to the other are multiple, subtle, and sometimes incompatible: it is a complex conversation about a complex topic in a broad context.

---

[3] Not to mention that in the USA wilderness has a highly specific legal definition that does not correspond exactly to any of the designations in Canada.

The role and influence of context has recently been raised in relation to environmental issues and concerns. In OR, Swinnerton has recognized that "the importance of ensuring the social and economic sustainability of local populations requires that balancing recreation and conservation interests must be examined within a much wider conceptual and operational context."[4] Engagement with this wider conceptual context entails explorations in the literature of the natural sciences, the social sciences, and social theory. For example, scholars have begun to investigate the connections between ecological integrity and social equity. The ways that wilderness protection intersects with poverty are indicative of such an approach.[5] Closely related are cross-cultural questions about the appropriateness of a Euro-North American model of protected areas for the local populations that live on or near such areas. Both these approaches can be grouped together under the rubric of political ecology.[6]

I situate my own work in the same general field of political ecology, but instead of addressing poverty or cross-cultural issues, I explore how a largely scientific understanding of nature influences our use of it. Thus, I take the wider context Swinnerton advocates as a political one in the broad sense that protected areas and their use are aspects of Canadian society that are embedded within a matrix of power, material effects on people's lives, and concrete ecological problems. Decisions about these areas and our use of them have powerful implications for the land, people, and society. In addition to understanding context in this way, I employ an historical approach that traces the wider historical and social context that both ecology and OR share. This shared context influenced their conceptualization of environmental problems and solutions.

---

[4] Swinnerton (1999), 199.

[5] For example, see the edited volume called *Producing nature and poverty in Africa* by Broch-Due and Schroeder (2000).

[6] See the recent edited volumes Biersack and Greenberg (2006), Paulson and Gezon (2005), and Peet and Watts (2004).

There have also been developments in social theory that bear on the concerns of balancing of recreation demand and conservation in a larger context. These developments recognize that the modernist separation of the natural world from the human cultural one is neither desirable nor feasible. When it comes to wilderness protection, it is now clear that the environment and human culture should not be conceptualized as separate things.[7] Furthermore, it is not just 'culture' in a generic sense, but specific sites of articulation between culture and the environment that need to be considered. That is, generalizations about the types of interactions between nature and any one culture must be tempered with details specific to each site. Not everyone reacts to or interacts with nature in the same way, regardless of whether they share a similar cultural background. Detailed discussions between ecology, outdoor recreation, environmental history, and social theory would be mutually beneficial and, moreover, are necessary for understanding the complexities of our interactions with nature. Such a conversation, however, has rarely occurred.

I endeavour to clarify and expand our understanding of the ways science influences our conceptualization of nature, the ways ecology influences OR, and the insights that social theory offers to help shift the discussion to a more productive space where new contributions can emerge, new alliances can be built, various perspectives can interact, and new processes for thinking about these issues can develop. If outdoor recreation, ecology, and environmental history wish to expand beyond the confines of current paradigms of thought (as I am arguing they should), a multi-dimensional conversational space needs to be crafted that respects a diversity of perspectives and methods.

---

[7] Much research and writing has been devoted to showing the cultural construction of wilderness for instance. See Cronon (1995b).

The crux of the issues that concern this work is the human/nature split. When it comes to outdoor recreation, environmental history, and the natural sciences, the majority of arguments and positions tacitly (or overtly) support the idea of an objective, external natural nonhuman world. In the relatively rare cases where these disciplines directly engage questions about the nature of reality, they usually take issue with poststructural accounts of the world as constructed or produced by humans. The debate over real versus constructed reality is an instance where contradictory and yet seemingly correct positions can be maintained. Regardless of the perspective one adopts, productive discussions depend upon an understanding of other perspectives as well as the contributions they can make. Thus, it follows that the confusion surrounding the relationship between social theory (specifically, poststructural theory) and environmental problems needs to be addressed. It is not the case, as has been suggested, that poststructural theory is anathema to environmental issues, outdoor recreation, or ecology.

The Nature of Theory: A Theory of Nature

> *... a language is both a map of the world and its own world,*
> *with its own shadowlands and crevasses—places where statements*
> *that seem to obey all the language's rules are nevertheless impossible to deal with.*
> *~ David Foster Wallace*[8]

While it is the case lately that more and more of the debate surrounding environmental issues takes up postmodernism and/or poststructuralism,[9] some of this debate is unproductively polemical. Poststructuralism is often the misunderstood object of vitriolic attacks.[10] I have often wondered what it is about this brand of social theory that produces such reactions. It seems there is something threatening about it. Kristin Asdal, in a similar vein, suggests that poststructural theory is feared by most environmental historians because of its "supposed potential for pushing historical analyses even further away from concerns for our physical world."[11] Donald Worster, arguably one of the most influential environmental historians, makes this concern plain. It is an upside-down proposition, he says, to claim that the word comes before the thing itself. For Worster, anything that posits words as more powerful than reality or, even worse, anything that suggests 'reality' might be only a word (thus deserving of the quotation marks) is backwards or upside-down. Words come (a distant) second to reality. There are things, Worster claims, that have happened (and are happening) that cannot be reduced to mere text. Any attempt to do so robs reality of its power as arbiter of truth—in fact, it removes all arbiters of truth and we end with a relativistic morass where everyone is entitled to his or her own truth.[12]

---

[8] Wallace (2003), 30.

For Worster, reality is the basis for knowledge—without it we are lost. This reality is eternal; it is the solid foundation upon which we build our knowledge of the world and our theories affect it not. For this reason, Worster claims that "many

---

[9] See Swinnerton (1999) who notes that "[m]uch of the current debate over environmental problems and approaches to the ecological crisis stems from the application of a postmodern philosophy view of science," 203. It should be noted that although I use the term poststructuralism instead of postmodernism the differences are subtle enough that for now they can be treated as interchangeable. The term poststructuralism is more pertinent to my work for reasons that will become clear later and so I use it here for consistency's sake.

[10] For example see McNeill (2003), Myerson (2001), Schatzki (2003), and Worster (1993, 1995a, and 1994). In outdoor recreation, scholars engage with postmodern perspectives less often. This could very well be because of the dominant view of wilderness is that it is something really real, something outside culture, something independent of humanity. Given this perspective, it is not surprising that postmodern positions are not common in OR. See Welton (1987) for an expression of this idea that wilderness lies beyond culture.

[11] Asdal (2003), 62.

[12] Worster (1994).

contemporary postmodern historians … [subscribe to an] excessive relativism [that] may distort reality"[13] Worster and others fear that when the question concerns ecological crises, such as species extinction or acid rain, any loss of reality's power to arbitrate truth will be devastating. When we lose sight of what is actually there, when we confuse reality with text the danger is that we will begin to see that "[a] landscape riddled with open cast mines, bleeding acid into streams, is as 'natural' as any other."[14] There are some things that are real and remain constant, no matter who views them; these things are true and real.[15]

If we adopt a poststructural perspective, these authors argue, we will not be able to find common ground to act together. Everything dissolves into random, changing, relative subjectivities with little meaning for each other. Nothing is real and true. How, then, can we determine true from false, or real from textual? As Ermath already noted, however, "[t]hese questions are entirely understandable, but they are questions formulated by modernity, for modernity."[16] The fear that we might not know what is actually going on in nature (that is, whether or not there really is an ecological crisis in Canada's national parks), and will therefore be much less able to effect change, drives many reactions to poststructural social theory.

My position vis-à-vis poststructuralism is similar to what Shatz meant when he said that Derrida's "distrust [of Enlightenment metaphysics] was that of a lover, not a prosecutor."[17] I believe poststructural social theory has given us some important insights;

---

[13] Worster (1995b), ix.

[14] Worster (1993), 176.

[15] In addition to environmental historians like Worster, these same concerns over poststructural theory are echoed by some historians of science (for example, Bowler and Morus [2005] and Russell [2005]), and certain environmental philosophers (for instance, Shepard [1995]).

[16] Ermath (2001), 52.

[17] Shatz (2004), 6. Accessed March 3, 2008.

yet, like all things, it has its limits. I would like to challenge it to grow and change precisely because of my indebtedness to it. This work, then, presents a history of ecology and its links to outdoor recreation that draws from poststructural social theory in some instances, yet also challenges and changes some of its tenets. My desires are to help OR and ecology think differently about some of the very foundations of their fields; to further the development of social theory in such a way that calling it *social* theory will be somewhat of a misnomer; and to contribute to the fields of history of science and environmental history. I will pursue this third aim by (a) examining ecology and OR through a theoretical architecture distinct from ones used by those who have already written about ecology's history,[18] (b) showing that some of the ways ecological science has historically structured knowledge have raised some philosophical and practical problems, and (c) offering an alternative process or orientation that ecology and OR could adopt that allows for more discussion and better understanding of the diversity of perspectives involved in the attempt to protect wilderness landscapes.

Archaeology and Genealogy

> *Historians' exercise great power and some of them know it. They recreate the past, changing it to fit their own interpretations. Thus, they change the future as well.*
> *~ Frank Herbert*[19]

Archaeology and genealogy form key theoretical elements in this work. Both of these are types of historical analyses that differ from chronologies that outline major themes and debates and which, in any case, have already been written regarding ecology. These approaches are distinct, but also they are compatible with each other. Foucault has distinguished between an archaeological and a genealogical history and this distinction forms a core element in the following analysis I apply to ecology and OR. I use archaeology mainly as a means to understand the discursive structure and practices of ecological knowledge production. From this vantage point, I then examine OR in using both archaeology and genealogy. I describe the discursive structure and practices of OR (archaeology), but then continue with an analysis of the lines of descent that connect OR to ecology and science in general. This second component employs more of a genealogical lens as it weaves issues of power, broadly defined, into a discussion of outdoor recreation discourse.

---

[18] For histories of ecology/natural science, I refer the reader to Bowler and Morus (2005), Bramwell (1989), Egerton (1977), Flader (1994), Golley (1993), Hagen (1992), Kingsland (1985), Martin (2004), Mitman (1992), and Worster (1993, 1994, and 1995a, 1995b).
[19] Herbert (1984), 371.

According to Foucault, genealogy focuses upon power relations rather than mainly upon language, which is more the domain of archaeology. "From this," Foucault said,

> follows a refusal of analyses couched in terms of the symbolic field or the domain of signifying structures, and a recourse to analyses in terms of the genealogy of relations of force, strategic developments, and tactics. Here I believe one's point of reference should not be to the great models of language (*langue*) and signs, but to that of war and battle.... History ... is intelligible and should be susceptible of analysis down to the smallest detail—but this is in accordance with the intelligibility of struggles, of strategies and tactics.[20]

For Foucault, archaeology, with its emphasis on discursive organization and structure, does not give enough consideration to power relations and lines of descent. In other words, history is not simply text and words. This is evidence that Foucault (and other poststructuralists) did not think only in terms of texts. What he believed, I think, is that words cannot be separated into empty, harmless signifying elements; words always come embedded into systems of power that have consequences.

---

[20] Foucault (1980), 114.

It is this larger system that Foucault turned to in his later work. In place of text and language, Foucault proposed that we need to think not in terms of epistemes (narrowly described as 'discursive'), but in terms of a more general apparatus that

> 'is both discursive and non-discursive.' … With the new emphasis, there is a shift away from the notions of the epistemic frameworks existing *in idea*, and a shift towards materialism. Power is to be directly related to bodies: 'What I am after is to try and show how the relations of power are able to pass materially into the very density of bodies without even having to be relayed by the representation of subjects'[21]

Said another way, genealogy "looks behind discursive practices to their extradiscursive setting, to the milieux from which they are excluded or in which their products are developed."[22] Thus the analysis I pursue first considers the discursive practices, but then moves to consider how those practices are incorporated into power relations that structure interactions with our environment. Ecology and OR are both discursive practices, certainly, but there is more to them than simply words. Both these discourses are implicated in how humans interact with and understand wild nature. These interactions in turn have material consequences. Genealogy, with its emphasis on power relations, provides a means to investigate the descent of influence from ecology (and science more generally) to OR as well as a means to understand the consequences of such relations.

A genealogy of OR highlights the ways that power infuses both human bodies and wilderness areas; it marks them, invests them, changes them, makes them grow, gives them life, but also assigns them death. Moreover, there is no autonomous, rational self relaying and directing this power. In fact, Foucault famously proclaimed that because of the way modern power operates, it is incorrect to continue referring to a founding subject: man, the self, is a recent invention and will soon give way to something else. Although he came to regret making such a bold claim, the idea that power operates without a pilot, precisely because it operates everywhere, necessitates looking beyond a sovereign that is the source of power. Thus, power becomes discursive; it is the power/knowledge nexus that pervades our bodies, minds, and the natural world. However, it must be kept in the forefront that what was done away with here was the founding, self-contained, autonomous self, not the material effects of power. Simply because power operates without a pilot does not mean it has no real effects. It means that what we might be tempted to think of as the pilot (one's self) is, in a significant sense, actually a product of power. That is, we do not use power without power also using us. The modern self is enmeshed within the modern operation of power/knowledge.

---

[21] Foucault quoted in Harland (1988), 156.

[22] Faubion (1998), xxxiv.

When so conceived, power always brings with it the possibility of resistance. When power circulates through people, the opportunity arises for individuals and groups to exercise power in ways that deviate from the dominant norms. Thus, any exercise of power is never complete or finished. It remains an ongoing process that is always open to being overturned. Resistance becomes salient, to take one example, in discussions of how MI protocols are disseminated to and through backcountry users. Locating the pilot who directs these messages and metes out the punishments for infractions is a futile effort. This deployment of modern power is too diffuse for such an analysis. The same holds true for resistance. Resistance, under these conditions, becomes a matter of micro-politics. The ways in which one conducts oneself, the frameworks in which one engages, and the material conditions one changes or with which one aligns oneself all manifest the micro-politics of resistance and power. To consider power, then, is to consider (potential) resistance to it.

Throughout this discussion on power, though, the material world has not disappeared; it is one of the main sites for deploying and contesting power, hence the need to consider the discursive and also the non-discursive in a genealogy. In this way, genealogy does not so much replace archaeology as extend it. Genealogy delves into the relations between discourse and physical or material bodies—what I am calling the extradiscursive or non-discursive.[23] It looks to materiality in the relations of power that govern discourses (such as ecology and outdoor recreation). These relations structure and mark bodies; they make them perform; they shape practices; and they construct buildings, institutions, and national parks and other protected areas while arranging relationships between materials within them. Much of Foucault's work was, in fact, a consideration of how the discursive affected the extradiscursive (for example, how the power of the discourse on sex marked bodies, directed practices, altered physiologies and genetic codes, and constructed and arranged buildings that housed/produced sex and sexuality). Given this emphasis on the non-discursive, one would expect genealogy to fit well with a history of ecology and OR in both their physicality and discursivity.

However, Foucault dealt largely with human discourse and human materiality (mostly human bodies, but also the shape, structure, tools, and organisation of clinics, prisons, schools, and military camps). He placed much less emphasis on relations of power that included nonhuman materiality. Even when he spoke of nonhumans, nature, natural history, or biology (e.g., *The Archaeology of Knowledge & The Discourse on Language,* and *Power/Knowledge: Selected Interviews and Other Writings, 1972-1977*), Foucault took the perspective of the human discursive domain. More often than not, he investigated how the sciences, as discourses, altered practices, bodies, and relations. He asked, for instance, "[h]ow was the subject established, at different moments and in different institutional contexts, as a possible, desirable, or even indispensable object of knowledge?"[24] In short, he took as his start or finish the human subject as it is produced by and produces discourse and materiality. So, for genealogy to include nonhuman materiality (such as is necessary for a genealogy of OR), some rotation is required to move the human figure off-centre. This rotation relates to my second aim: to develop 'social' theory such that the adjective becomes inappropriate—our theories need to account for nonhuman materiality and bodies, not just the human or social realm. In the process, I present a novel and significant alteration to poststructural theory that warrants a more detailed examination. Before that, however, another aspect of poststructural theory needs explication.

---

[23] It should be noted that in his use of the term 'discursive' Foucault meant something more than simply words and texts. The mistake of thinking that discourse refers only to textual elements was not common in the 1960s and 1970s. So Foucault did not have to make the same kinds of distinctions one does today in order to avoid the kinds of criticisms often levelled against poststructural social theory. Consequently, I am using two terms where Foucault often used only one.

Poststructural Agency

The limits of my language mean the limits of my world.
~ Ludwig Wittgenstein, 'Tractatus logico-philosophicus'

The question of agency is another point where Foucault emphasized humans. For Foucault, agency, resistance, subjects and subjectifying practices, objects and objectifying practices, and discourse are interrelated. The subject that Foucault referred to was certainly not the autonomous, ahistorical subject: "Foucault, in his genealogy of the modern subject, seeks by a genealogical inquiry into the construction of subjectivity to demonstrate that subjectivity is a historical construct which cannot be understood outside relations of power and knowledge."[25] With respect to subjects and identity, we might say that the purpose of genealogical history is

---

[24] Joseph (2001), 87.

> not to discover the root of our identity, but to commit itself to its dissipation. It does not seek to define our unique threshold of emergence, the homeland to which metaphysicians promise a return; it seeks to make visible all of those discontinuities that cross us.[26]

Because genealogy shows that identity is dissipated and that the subject is not only split but partly produced by discourse,[27] the agent(s) of change, obviously, cannot solely be autonomous human beings in full control of themselves. This effect on identity and the subject raises the problem of terminology: I choose to use 'subject' to indicate this splitting and dissipation, and 'identity' to refer to the mistaken modernist idea of the self-contained unified person. As Catherine Belsey notes, "'[i]dentity' implies sameness: that's what the word means. Subjects can differ—even from themselves."[28]

To the extent that humans are agents, this agent is articulated through, subjected to, and often objectified by discourse at the same time as it uses discourse. Whenever I speak, "I reproduce (or challenge) the ruling ideology … and I am in that sense a source of initiations, actions, decisions, choices. But at the same time the subject is *subjected* to the meanings and sentence structure that language permits."[29] This subjection connects resistance to power: I can resist, initiate, challenge, and change dominant discourses, but, at the same time, I do so within or against ways that discourse allows. In either case, power and resistance are intimately entwined. Thus, I am spoken into being; I am subjectified by and through my use of discourse. In this sense, discourse is both enabling and constraining. Both of these features, moreover, are productive: constraints give shape to what acts at the same time as the opportunities that discourse affords are operating. Discursive constraining and enabling forces are not a matter of freedom of choice—I do not get to determine the discourses that operate on me. For the most part, I fill subject positions that already exist in the discourses. For example, "Western culture decrees that there are two sexes; the English language, as the inscription of a culture, offers two pronouns, one masculine and one feminine, and subjects are expected to identify with one or the other. The most scrupulously non-sexist parents have no choice but to speak of their children as 'he' or 'she,' and children generally do their best to become what language tells them they are."[30] The further I move outside these subject positions, the less understandable I become and the higher the potential cost. Agents, then, are bound tightly into complex webs of power/knowledge, practices, and behaviours. When we speak of resistance under these conditions, we must see it as focussed against the features of discourse instead of as an act of individual autonomy.

---

[25] Berard (1999), 217.
[26] Dreyfus (2004), paragraph 37. Accessed March 14, 2008.
[27] Foucault (1980), 114-117.
[28] Belsey (2002), 52.
[29] Belsey (2002), 37.

If subjectification by discourse is unavoidable, objectification is not. To speak is to be subjected to discourse; to be objectified by discourse is to diminish people's humanity by reducing them to an object. Resistance, then, is directed at eliminating objectification, not subjectification. As we shall see, however, objectification need not be seen only in this light. When we think exclusively in terms of builders and what they build, subjects and objects, or active and passive we continue to conceptualize agency in relation to humans and see all forms of objectification as negative. This position, unfortunately, does not make any comment on the process, value, or place of objectifying things. It is 'self-evident' that things are objects of power and discourse; they can never be subjects, nor can they be subjected to discourse. Objectifying things, unlike objectifying people, is not seen as negative. However, this view may not, in fact, be accurate.

---

[30] Belsey (2002), 50.

Acting Like A Turnip

> *Nature, which they say is without imagination and without reason, has contemplation in itself, and produces what it produces by the contemplation which it 'does not have.'* ~ *Plotinus*

As complex as Foucault's discussion of agents, subjects, subjectification and objectification was, it was, nevertheless, a primarily human subject, or agent, or object that he addressed. Part of the reason for this limitation is bound up with Foucault's two modalities of power—sovereign and disciplinary—both of which privilege human subjects/objects. Humans both stage and view the spectacle of sovereign power, and humans are both created by and exercise disciplinary power. Under disciplinary power, which is the modality that began with modernity, one can never be only a subject of knowledge that simply acts; we are always subjected at the same time. However, this subjectification is never complete; there always remains the possibility of resistance, which is to say, the potential for agency and subject-hood. While humans, for Foucault, were not the rational, self-contained entities modernity claimed, they were also not completely produced and contained within discourses not of their making. If they were, it would be pointless to speak of resistance. To say it more concisely, there cannot be resistance without the possibility for agency, and where there is agency, for Foucault, there is, in some form(s) or other(s), a human subject.

Questions of subject and subjectivity play a prominent role in Foucault's work; consequently, he does not ask if there are things that can be only objects of power. It is more or less assumed that nonhumans are objects of discourses. However,

> [m]ore recently, environmental historians have argued that nature too has agency. This claim often has been met with skepticism. After all, the argument goes, although nature may resist and complicate human actions, producing all sorts of unintended consequences, nature has neither the intentionality nor the choice that humans do.[31]

Environmental historians are posing such questions and not assuming that the answer is obvious. Although Foucault did not directly address this issue, we can ask, 'are nonhumans always only objects of human discursive power?' If objectification entails a reduction in humanity, then nonhumans, by their very definition, must be completely objectified. As Belsey put it,

> [p]oststructural theory suggests that precisely because meanings are constructed, they can also be contested. Foucault stresses throughout his work the possibility of resistance, since power is always power *over* something or someone capable of disobeying. (No one claims power over turnips.) In Foucault's model, power is mobile, flexible, transferable. Both his position and Derrida's, in their distinct ways, imply choice and responsibility, ethical and political.[32]

So, poststructural theory is not necessarily relativistic, amoral, or apolitical. Opportunities for resistance exist, so exercising resistance is a question of ethics and politics as much as power.

However, poststructural theory makes resistance and agency human-centred. Only people are capable of disobeying. Hence, it makes no sense to claim power over turnips.

---

[31] Nash (2005), paragraph 1. Accessed March 14, 2008.
[32] Belsey (2002), 89.

And yet, three troubling things remain: (1) The world is more interconnected than this claim would lead us to believe. Objects are implicated in power relations in many complex ways; they should not be thought of as simple inert objects. For instance, turnips may well be linked with war in numerous ways: destroying them as an attack on a food source, or supporting genetic modification technology that can be used to develop/test biological weapons. (2) Even if one takes turnips apart from relations such as these, they are not completely malleable objects. They will not become whatever we want: they are far better cooked and served with dinner than as a substitute for a tent on a cold rainy night. (3) They also act on the people who use them, imparting a subject-hood associated with English and European culture.

Granted, turnips, if they have any agency at all, do not manifest it in the same configuration as humans do. But still, the conclusion that turnips have no agency at all was not arrived at through careful theorizing. In her case, Belsey's comment about turnips is parenthetical, designed more to elicit a chuckle than a carefully thought through statement of her position. It came about almost by default: it is the result of the starting points in poststructural theory, Foucault's in particular. It is not the result of an overt theorizing about the place of nonhumans in the power/knowledge nexus. By rotating Foucault's emphasis off human subjects, we can challenge this omission and explore the role that nonhuman agents have in sharing agency. It should be noted at this point that, whether or not the agent is human or nonhuman, the concept 'agent' is misleading as it suggests an internally consistent structure (in the sense of self-conscious entities fully aware of and in control of their own motives and intentions). Just as there are no pure subjects of knowledge, there are no pure agents totally in control of themselves. In an effort to spread the concept of agent wider, I adopt the term 'actant,' which reminds us that human beings are not modern, self-aware agents and that humans are not the only player in the drama.[33] I am suggesting that all kinds of actants participate in the creation of meaning and signifying practices in varying ways.

We need to qualify the general scope of agency. Foucault helps with this qualification because he reminds us that no agent is fully in command as an independent source of power. In addition, though, we need a way of extending agency beyond humans. Why are people the only entities that are allowed to be subjects of, subjected to, and objectified by discourse? Why couldn't nonhumans share somewhat in these processes, as well? As long as we exclude nonhumans, we remain largely entrapped in the agent-object binary where the only allowable manifestations are human subjects, nonhuman objects, human objects, and subjectified humans; there is no place for nonhuman subjects or subjectified nonhumans. If conflict/contestation is ubiquitous why should we presume that either we choose only among human actors or that only humans are affected?

Elizabeth Ermath discusses agency in the discursive condition and wonders what human agency would be like if humans are somewhat products of discourse. Her construction of what agency means speaks to some of the questions I raise here. For Ermath, "[a]gency in these contemporary conditions is not a singularity but a process, a happening, a particular expression of systemic value." She returns to Saussure, who noted that "'identity' and 'agency' appear not as discrete essentials but as the practices of a life long activity of specifying the available rule regimes." She concludes that we should think of agency in this discursive condition as "no longer … only a subject-in-process, or even a subjectivity-in-process, but something more like subjectivity-in-process*es*."[34] There are, she notes correctly, many different processes at work that are never fully completed. Thus, agency is not possessed by an actor; rather, it is constantly performed by one in a variety of fashions. I would add that we should think of it more as 'subjectivit*ies*-in-process*es*.' Ermath is correct: agency is not just a singularity, but it is not just a multiplicity of processes, either. Numerous subjectivities are crafted for ourselves as we engage with and are subjected to different discourses throughout our lives. Finally, agency is not related solely to human subjects: there is a plurality of subjectivities engaged in constitutive agentic processes. The archaeology/genealogy that I propose, consequently, must attend to the variety of actants involved.

---

[33] See Latour (1999) for a more detailed discussion of this concept.

Writing a Rotated History

Now time itself becomes history dependent.
~ Ilya Prigogine (Nobel Laureate in physics on the implications of chaos theory.)

What, then, are the consequences of combining archaeology and genealogy with an expanded understanding of discourse and agency? Even from a more 'standard' perspective (the one, that is, that focuses more on human power relations) certain things become apparent. This history cannot be simply a recounting of the developments in ecology and the ways in which they were or were not adopted by OR. The relations of force that genealogy seeks to understand are not synonymous with a chronicle of events. Consequently, I am interested in the rules that govern the production of knowledge in both ecology and OR. I wish to know at what level of organisation ecology and OR operate: Who can speak, in what ways, with what authority, when, and about what?

---

[34] Ermath (2001), 46.

These questions constitute the archaeological portion of the analysis. In addition, I am interested in the material power relations that ecology and OR have structured over time: How are the various tactics of power deployed in the material world, and what effects do these have on humans and nonhumans? In other words, what kind of nature and human self have ecology and OR built? Answering these questions necessitates understanding both the relations between ecology and OR that have been influential in terms of minimum impact practices and policies, and the development and debate over the goals and objectives for wilderness areas. I am interested in how these relations have been influential and what kind of human subjects were built, positioned, and marginalized by the rules of formation in ecology and OR. These types of questions constitute the 'standard' material of an archaeology or genealogy.[35]

On top of these 'standard' questions, I also include questions of causality, nonhuman actants, and paradoxes in ways beyond those that archaeology and genealogy have ordinarily done. I want to know what human power relations would look like 'from the other side,' so to speak. Answering these questions requires that we know how the trace of nonhuman agency operates in both ecology and OR. Leaving out this trace has serious consequences in terms of limiting how we understand the world and our place in it. Knowing who or what can have agency and in what ways, and what effects these actants can bring about or assist in bringing about defines who can act and with what authority. Thus, agency is a central problem and has implications for ecology and OR that I will highlight. Understanding agency in the way I suggest allows for a re-examination of the way knowledge is produced and used in both ecology and OR. We can begin to take account of and change certain inconsistencies and incompatibilities that have until now baffled scientists and OR scholars.

---

[35] I do not wish this comment to be read as saying that there is a particular set of questions that all archaeologies and genealogies will address. I wish to suggest that archaeologies or genealogies focus on different questions and offer another type of analysis than narrative history.

For this kind of analysis to work, then, a twofold shift is required: first, we must expand our notion of discourse to include extra-discursivity or materiality; and, second, we must expand our understanding of the actants involved in the process of history. I propose to make such expansions and write the history of ecology's links with OR, specifically minimum impact practices and management goals. From this perspective, discussions between ecology, OR, environmental history, and (social) theory can begin to extend the scope and value of who participates.

The other component I include that goes beyond a strict archaeology or genealogy is that of paradoxes. Paradoxes are a component of much of the poststructural work of Derrida and others; yet, in ecology and OR the power of this concept is not often explored. In some ways, paradoxes in ecology and OR are taken as self-evident. Many authors, for instance, have commented on the paradox of loving wilderness to death. Other times, paradoxes are treated as traps to avoid or curiosities to be mentioned instead of fecund elements worth exploring. I wish to challenge the assumption that paradoxes are obvious or simple. Rather than either dismissing ecology and OR because they are paradoxical or dismissing the paradoxes in them, I see paradoxes as fruitful areas to engage.

The Issue of Language: The Politics of Writing (and) History

I try to tell them that all words are plastic. Word images begin to distort in the instance of utterance. Ideas imbedded in a language require that particular language for expression. This is the very essence of the meaning within the word exotic.... Dangers lurk in all systems. Systems incorporate the unexamined beliefs of their creators. Adopt a system, accept its beliefs, and you help strengthen the resistance to change.
~ Frank Herbert[36]

When it comes to writing history or generating scientific knowledge, some significant changes result from the preceding comments. This section builds an argument for what these changes are and how they affect our approach to writing history and creating knowledge. It should be obvious by this point that many poststructural analyses focus on language. The question of whether or not we can experience something that we cannot, under any circumstances, translate into language proves more difficult to answer than might first appear. What is a non-discursive experience? Although "Haraway makes the pertinent point that there can be no pre-discursive encounter with biology or, more generally, nature,"[37] I wish to question whether this point necessarily rules out all pre-discursive encounters. If every time we speak or think we engage a linguistic lexicon, does it follow that language places absolute restrictions on our experiences of reality? If no one anywhere at any time can speak or think of it, is it real? These questions, and others, are the focus of much poststructural theory. As Ermath notes,

> [w]here once there was a distinction between language and so-called Reality, now language constitutes realities so that the term 'reality' must wear quotes to indicate its systemic function. Inhabiting a language means inhabiting a reality, and that so-called 'reality' (one begins to search for ways to qualify such a once-unproblematic term) changes with the

---

[36] Herbert (1981), 342.
[37] Howell (1996), 141.

> language. Complexity increases with the fact that one inhabits several or many languages simultaneously.[38]

Part of the argument I make is that language is not simply a tool we use, but is part of the very structure of the problem.

According to poststructural theory, discourse cannot function simply as a set of signs that relate directly to an external reality. The word or sound (called the signifier) does not refer univocally to a corresponding image or concept (the signified); furthermore, the sign (the combination of the signified and signifier) does not relate directly to one piece of reality (the referent). For example, the word 'apple' (signifier), the concept or image of one (the signified), and the actual fruit we like to eat (the referent) are often thought (outside of poststructural circles, that is) to be directly linked. Were this to be the case, language would become merely labels that correspond to reality. However, what the signifier points to is actually another signifier. When we read the above example, there is no actual fruit to be had on the page. The word 'apple' referred instead to the written statement 'the fruit we like to eat.' Moreover, the type of apple in someone's mind, its size, colour, shape, purpose, and so on, is not guaranteed to correspond perfectly to someone else's. This vagueness is more than just a matter of semantics. We might think that at some point we would be able to get to the bottom, to actually find the elusive apple-as-a-thing without its corresponding signifier or signified; instead, we always only encounter the sign function. To encounter a pure referent or signified is to have a non-discursive experience, which makes no sense.

All our concepts and images are expressed in an already existing language.[39] All our encounters with the world of referents are likewise coded through some type of signification system. “Poststructuralism proposes that the distinctions we make are not necessarily given by the world around us, but are instead produced by the symbolizing systems we learn.”[40] In other words, because the language systems we use are not of our making, because they exist before we do, and independently of ourselves, we cannot control their exact shape and functioning. In the end, we are able only to express that which language has some capacity for expressing. If it cannot appear in language anywhere, it cannot be expressed (to others or ourselves).

---

[38] Ermath (2001), 42-43.

Language, for all its power and flexibility, is not a perfect tool of infinite variability. We are not free to make any meanings we like, not because language will actively resist us in these attempts, but because it has already entered into the picture as the very thing through which we produce meanings in the first place (both the novel and the enervated). Language is most often the conditions of thought rather than an object of thought. Language is productive, absolutely, and this productivity is the very heart of the matter. Language (all signification practices, in fact) gives the very possibility of making meaning. This possibility has a particular configuration in rational, western, metaphysical signification systems. The meanings we make with this configuration are not pre-ordained by this configuration; they are enabled by it. By way of analogy, individual basketball players' moves are not pre-ordained by the rules of the game. Instead, players' moves are enabled and constrained by the rules of the game. Just as there are rules in basketball, so too are there rules for languages that yield the possibility of producing

---

[39] Language in the sense I mean here refers to all forms of signification. It includes, for example, sign language, all spoken and written languages, pictorial languages, and symbolic language.
[40] Belsey (2002), 7.

certain meanings. Language produces the possibility for a range of meanings; it does not actually produce those meanings.[41]

The consequence of this ability of language to give the potential for meaning lies not in productivity itself but in the timing of its deployment. Language presents the *possibility* to produce meaning, which means that it has already entered the picture as the system by which we will be producing meaning. In this sense, language forms a type of contract that we all sign, one that says that we agree to interface with the world through the vehicle of a discourse. There is something unique about this contract, as Harland notes:

> [t]he special feature of this 'contract' is that no one ever gets the chance to evaluate it before signing. The individual absorbs language before he can think for himself: indeed, the absorption of language is the very condition of being able to think for himself. The individual can reject particular knowledges that society explicitly teaches him, he can throw off particular beliefs that society forcibly imposes on him—but he has always already accepted the words through which such knowledges and beliefs were communicated to him.[42]

In other words, we may resist components of discourse but only by using (some of) the very system that produced those elements we are now resisting. When we engage language, we adopt its restrictions too. If we wish to challenge these restrictions, we encounter a difficult situation: the very form of exceeding these restrictions is couched in the thing that we are trying to exceed. Thus, all forms of resistance, to be meaningful, presuppose discourse; they are mounted from within it. In this way, discourse is always already operating prior to resistance.

---

[41] Shogan's article offers a very interesting discussion of the productivity of language in the context of the rules of basketball (Shogan, 2002). She highlights for the reader the productive enabling and constraining elements of the game. One of her points, one that is germane to this discussion, is that even the constraining rules are helpful in making the game run smoothly. The same is true of all discourse: limiting functions are equally important as enabling functions. When Foucault said that discourse (power-knowledge) was productive (or, stated in the reverse, power is not simply oppressive), he included both the positive functions of productivity as well as the negative (constraining) ones (Foucault, 1980).

[42] Harland (1988), 12-13.

The issue of linguistic limits becomes evident particularly in relation to the concepts *natural* and *cultural*. If, as I will suggest, humans and nature never really were split and they actually cannot really be, what do we do when the language system we operate in continually attempts this splitting? It may not be successful in its attempt to separate humans from nature, certainly, but it is still incompatible with the way I wish to theorize here. Unfortunately, language is the only tool I have with which to theorize: I am stuck with it. The question now becomes, 'how can I alter my language to allow me to think/say what I want while still being intelligible?' The quest for intelligibility, in this sense, is exactly the normalization process. The more I adhere to the norms of language, the more comprehensible I will be. In this way, writing is political. It serves to maintain itself, its norms, and its standards of operation. What I can say, how I can say it, and what I cannot say are all political in the sense of silencing some voices and privileging others. To challenge the politics of normalization is always difficult; when the normalization is of a linguistic type, it becomes even harder.

The problem is that language demands that I speak its language. If I do not, I am denied the power and use of it. And this denial amounts to a silencing so profound that I cannot even voice a criticism from within it. When I write, then, and it appears normal and understandable, my criticism has been so silenced that the only evidence of it is its lack. And yet this lack cannot be distinguished from instances where silence is merely a capitulation to the demands of language. Where is resistance in these instances? How can I resist using language as required? Where does such an attempt find and express itself?

The quest for intelligibility under these conditions will be an ongoing quest throughout this work.

The notion of signifiers, signifieds, and referents has led to other poststructural realisations. If we cannot master language and it is not a transparent process of pointing toward reality, then the meaning of any utterance or series of utterances will always exceed our intentions. This is the belief that "[t]he meaning that one grasps, and that is immediately manifest, is perhaps in reality only a lesser meaning that protects, confines, and yet in spite of everything transmits another meaning, the latter being at once the stronger meaning and the 'underlying' meaning."[43] It must be remembered that this excess, this underlying meaning, does not move from signifier to signified or referent (e.g., from 'apple' or 'pomme' to the real fruit we eat). Instead, the excess moves from signifier to signifier (e.g., from 'apple' to 'pomme' to 'the fruit we eat,' etcetera). Derrida explains this process as signifiers always pointing away from themselves. "In effect, signifying is nothing more or less than signifiers *in motion.*"[44] Derrida describes signifying as

> a state of *dissemination*. Here is no full rich harvest of signified meanings such as modern Anglo-Saxon literary critics might delight to find, but rather a kind of endless loss and spillage.... *Dissemination* must be distinguished from *univocity* or the state of single meanings maintained by the signified in the writer's mind; but it must also be distinguished from *polysemy* or the state of multiple meanings maintained by the signified in the reader's mind. *Dissemination* is the state of perpetually unfulfilled meaning that exists in the absence of all signifieds.[45]

Foucault, as well, commented on the excess of language, but in relation to its author. Under a structuralist or modernist conceptualisation of language and meaning, the author was thought to imbue words with intentions. Thus, the objective of literary criticism should lie in finding this intention or the true meaning based on the author's intention and remaining faithful to it.

---

[43] Foucault (1998a), 269.
[44] Harland (1988), 135.
[45] Harland (1988), 135.

As Foucault noted, however, when someone writes a text, it is a mistake to think of him or her as the sole author of that piece. When we do, the words of an historical text, for example, become the vehicle by which we think we gain access to that mysterious realm of intention. We try and search for this intention inside the head and heart of the author. We often try to give the author a deep motive, a creative power, or a design; however, these are but projections "of the operations we force texts to undergo, the connections we make, the traits we establish as pertinent, the continuities we recognize, or the exclusions we practice."[46] For Foucault, the author is a fictitious function that masks itself as a person with intention.

The author has served various functions over time and in various fields. For example, Foucault commented that

> in biology and medicine [as compared to literature] the indication of the author and the date of his work play a rather different role. It is not simply a manner of indicating the source, but of providing a certain index of 'reality' in relation to the techniques and objects of experience made use of in a particular period and in such-and-such a laboratory.[47]

So in biology, medicine, and also ecology I would argue, the author function serves to make the results appear truer by equating the relations of technique and objects of experience with reality. When it comes to the discourse of ecology and OR, searching for only the author's intended meaning ignores the spillage of meaning that he or she cannot contain. Intention can be stated, but stating it does not completely contain the dissemination of meaning. Thus, we need to read texts with an eye to uncovering this excess and to do so we must also exceed the author's (stated) intentions. Often this excess takes the form of a paradox that threatens to undo the author's intention. If the author's intention can be seen as the only true interpretation of a text, then the paradoxical excess that disseminates outward is contained. Therefore, learning to see and work with paradoxes is an important corollary to de-centering the author's intention. Once we step outside the restrictive notion of intention, we see a host of meanings, some of which appear self-contradictory or paradoxical.

---

[46] Foucault (1998c), 213-214.
[47] Foucault (1998c), 213.

In terms of a genealogy of ecology and its connections with OR, an awareness of paradoxes means I look for meanings that lie outside and often contradict what the author seemed to have intended. These are places where the text begins to unravel in spite of everything the author does to stabilize it. Far from being simply a contradiction, these points of excess are productive because they release us from the idea that there can be a single true and comprehensive interpretation (usually thought to be that of the author's intention). Of course, this understanding begs the question of how we should respond if everything we say contains and produces self-contradictory excesses. Rather than suggesting a solution, I suggest a process or orientation toward knowledge that allows for such spillage without it completely undermining all knowledge claims.

We must insert a number of caveats here: (1) Understanding this excess does not erase the meaning the author intended. Instead, it refuses to privilege this meaning as the truth. (2) Although the author's meaning exists, it cannot be known as such with an absolute degree of certainty by a reader. (3) Even in cases where an author is present to explain her intentions, this clarification cannot establish relations of truth; in other words, an author's stated intention does not reduce the dissemination of meaning to the point of one true meaning. (4) The idea of intention should not, in the end, be taken as a powerless or empty force. Even without the grounding of truth, intention does confer a level of persuasiveness to those readings that can be seen as falling in line with it. Thus, the meanings a particular ecological text might have for OR scholarship need to include the intended meaning, which may or may not be the one that seems to align most nearly with what the text says. Of course, other meanings must also be considered, but certainly we must include those meanings that suggest themselves as the author's intention.

Now we can approach texts differently. We can,

> instead of reconstituting the immanent secret, [treat] the text as a set of elements (words, metaphors, literary forms, groups of narratives) among which one can bring out absolutely new relations, insofar as they have not been controlled by the writer's design and are made possible only by the work itself as such.[48]

All texts contain new, unintended meanings. If meaning is not completely controlled by the writer, then texts can be read in surprising ways. The text will always mean more than it says. Its meaning exceeds itself. This excess is partially what Derrida called deconstruction: the excess that lies at the root of all texts and yet cannot be contained by that text.[49] Ecology, OR, and history comprise texts that now must be read twice:

---

[48] Foucault (1998b), 286-287.

[49] Deconstruction is a particularly difficult word to define or describe. In this example, I have left off any mention of the subject of the action. If deconstruction is a process, if 'to deconstruct' is an actual verb, then it must have a subject capable of doing it. Unfortunately, deconstruction is not an ordinary verb. More accurately, it is a verb that has itself as its subject. No one deconstructs: the excess in all texts, by definition of its being an *excess*, spills over. Rather than deal with the un-containable excesses and paradoxes, ecology and OR have directed attention onto other pressing concerns. I wish to re-direct attention back onto those excesses. I do not, it should be noted, deconstruct these fields in this process.

The mistrust of poststructural theory I spoke of above is connected to the difficulties associated with deconstruction. The logic of deconstruction is strange, because deconstruction is not just logical: it is also pre-logical or post-logical. Deconstruction is independent of logic to a large measure, and so it has a

> a first reading (a commentary or explication for example) which tries to be as faithful as possible to it, followed by a second reading whereby the text is subject to a series of interruptions which open up the various points of undecidability, moments of decision-making or *aporias*. In this way the text deconstructs itself through the tensions of its own fault-lines, its own interior inconsistencies.[50]

A text, whether historical or not, is always plagued by its own internal inconsistencies. The approach I take is not an attempt to resolve or cover them up. Instead, I seek them out. In particular, I search for the paradoxes that threaten the logical consistency of ecology and outdoor recreation. These paradoxes are most often found in the realm of silence. So, this history is not only one of developments and achievements, the aspects that stand up. I am also interested, probably even more so, in the instances where the text is silent about its own inconsistencies, inadequacies, and failings. It is in understanding these silent inconsistencies that ecology and OR can move toward new conceptualizations of wilderness, its place in society, and humanity's place inside wilderness.

In the end, the contributions of poststructural theory to this project are the reminders (1) that because language is not transparent—it has effects on what we see, think, and feel—we cannot look through language and see reality clearly and independently on the other side, (2) that signification does not necessarily link to any aspect(s) of reality (because of the spillage of meaning), (3) that power intervenes to produce what is taken as the truth in any discourse as well as those positions from which individuals are qualified to speak that truth (called subject positions), and (4) that discourse produces what constitutes the truth spoken from these subject positions by means of governing rules or practices (rules that also produce the false and the negative).

---

difficult relation to western metaphysics. As Lehman noted "deconstruction's tactics may be 'impish' and its logic 'absurdist,' but there is 'no denying the seriousness of its intent'" (quoted in Guilderson 1993, 146).

[50] Jenkins (2003), 26.

These points entail a refocusing off questions of how true any view in ecology or outdoor recreation might be and onto the functioning of the discourse that supports such an attitude. Furthermore, it brings to the fore questions of power: How does ecological science produce the truth about nature, and what are the effects of producing knowledge in such a manner? As we will see, ecology and OR are modern disciplines. As such, they have a modern orientation towards knowledge; consequently, that knowledge is often based on controlling, surveilling, penetrating, classifying, and managing nature. According to the modern view, this knowledge is produced by human agents who objectively observe an external and autonomous but highly objectified entity called the natural world. Through this process, ecologists and OR researchers discover what is seen as objective facts or the truth about that natural world. The important points for us at this stage are to recognize that this knowledge results from a particular set of practices and rules for the production of knowledge, that knowledge is built, not found, and that language is a powerful force in both creating and restricting the circulation of truth.

A Naïve Hypothesis

It was the absence of questions that made answers absolute—not knowledge!
~ *R. Scott Bakker*[51]

This project began with a simple question: is ecological knowledge reflected in OR literature, and, if so, how? From this point, I thought to explore existing links, offer some critiques, and suggest other possible linkages. My original hypothesis was that OR would be based on older conceptualizations of ecology that needed to be updated. For example, in 1987 Hammitt and Cole argued that change is the norm in nature and is called succession.[52] However, succession is not the ecological definition of change. Succession refers to a particular, sequential series of changes at a certain level of organisation with a certain goal or end state in mind. This is one case where OR used concepts from ecology in ways ecologists would likely contest.

---

[51] Bakker (2005), 132.

While in some regards it was true that ecological knowledge is simplified in OR literature, the story turned out to be far more complicated. The initial assumption that ecological knowledge would be reflected in OR may have been naïve, but it was not without reason that I assumed as much: this field frequently makes use of basic ecological terms and concepts (for example, succession, ecosystem, predator-prey relationship, vegetation cover, carrying capacity, and water quality).[53] An example of the way ecological knowledge appears in OR can be found in the 1999 theme issue of the *Journal of Experiential Education* on environmental education. Cain's introduction to this issue included the comment that it was necessary to raise the awareness of the ecological/environmental crisis. As a result of this introduction, one could assume, on the surface at least, that a few of the articles would contain some ecological content related to that crisis; however, none of them dealt substantively with the science behind this crisis. These articles dealt instead with increasing attachments to natural places or learning to love and respect nature. This idea of attachment to a natural place is quite common in OR. Often, researchers will investigate user preferences, perceptions of impacts or ecological damage, or values surrounding the protection of environmentally sensitive areas. This is important literature, for certain, but it does not replace the need for ecologically sound work to inform our choices and decisions. What people's values are, what their preferences are with regard to nature, may run counter to ecological principles.[54] Consequently, studying preferences or increasing attachments to natural areas may also place greater pressure on ecological processes and functions by encouraging more people to use these areas without understanding or valuing the ecology of the place.

---

[52] Hammitt and Cole (1987), 195-196.

[53] See, for example, Bélanger et al. (1990), Brissette, Haas, and Benson (2001), Burdge and Field (1972), Ewert, Place, and Sibthorp (2005), Hammitt (1983), Hendee and Dawson (2001), Hill and Shecter (1978), Jacobson (2001), Laven, Manning, and Krymkowski (2005), Marsh (1970), McGivney (1998 and 2003), Thapa and Graefe (2003), and Witt (1993).

Bunting illustrated both the lack of substantial ecological theory in OR as well as the premise that OR is based on older ecological models in the following statement: "[a]lthough there is a general understanding and acceptance of the concept of interdependency in nature, there has been little, if any, discussion of the relevance of that concept for adventure pursuits."[55] Bunting noted that ecology and environmental sciences had both accepted the idea of interdependency but OR had not. Interdependency is a cornerstone concept in many ecological studies. The fact that it does not appear in OR, even when the field expressly addresses impacts, is indicative of the shallow level of ecological sophistication in outdoor recreation. This level is significant for another reason. By 1990, when Bunting penned this comment, ecology had begun to move away from the notion of nature as interdependent in any simplistic way. Chaos theory, stochasticity (chance), and randomness were becoming more common in ecological literature. So, while Bunting was correct in noting that not much ecology had been incorporated into OR, her comment also belied the lag between ecological theory and OR's use of it.

---

[54] Cain (1999). For studies of user values and perceptions see Eagles (1985), Farrell, Hall, and White (2001), Kim and Shelby (2005), and Shafer, Hamilton Jr., and Schmidt (1969).
[55] Bunting (1990), 454.

As it turns out, the assertion that humans damage the ecology of natural areas is ubiquitous in OR, but the associated details and science surrounding that assertion are largely absent. Although repeated references are made to the damaging effects of humans on ecosystems, detailed, in-depth discussions of the ecology of those systems rarely accompany these references. Ecology forms the rationale, but not substantively, for minimum impact ethics and policies and education programs that teach backcountry users to leave behind as little evidence as possible of their passing. This has been noted by some, although its significance has received less comment. As recently as 2003, researchers commented that "[m]uch less, it seems, is wilderness valued for its personal or business utility, or even for its use in science. It seems more and more that ecological and existence values are central to Americans' viewpoint on wilderness."[56] In other words, wilderness is important for ecological reasons, but ecology itself does not seem as important. Again, one would initially think that if ecological values are increasing in importance, so too would the value of the science that contributes to our knowledge in this respect. However, it appears that public perceptions of the value of ecology for wilderness are static. That is, ecology is seen as already having contributed the important knowledge of wilderness; what remains now is to protect those aspects, functions, and elements that have been identified as central to the healthy functioning of wilderness, and ecology is not necessarily needed for this protection. Perhaps this explains why Greg Simmons could state that academic departments could benefit from a campus outdoor recreation program, but not the reverse. He noted that departments of natural resource management and of geography both needed to train their students in wilderness living and thus would be supportive of an outdoor recreation program at their university. However, the benefits ran only one way: outdoor programs could not, it seemed, benefit from these departments. Outdoor clubs and programs did not need to be concerned with detailed information about the ecology of the places they used, but the disciplines that produce such knowledge need to know all about OR program standards. Thus, the role that ecology plays is comparatively small in the university-level outdoor programs.[57]

---

[56] Cordell, Tarrant, and Green (2003), 31.

Although outdoor recreation has been a focus of attention in some manner or other for the whole of the twentieth century, by the 1960s the contours of the discussion had changed dramatically.[58] In the USA, after World War II, wilderness areas came to be seen as either a recreational resource or a scientific reserve. These two ideals clashed, and wilderness advocates argued with Forest Service and other government personnel over whether or not areas should be protected as biological reserves and strict limitations on visitors be imposed. "This debate became heated after the passage of the Wilderness Act [of 1964], and it tugged on the very stitching that held the wilderness idea together. These debates laid the groundwork for the rise of a minimal-impact camping ethic in the 1970s that would displace woodcraft as the dominant wilderness recreation ethic."[59] By the 1990s, minimum impact, largely in the form of the Leave No Trace! (LNT) program, had become the official ethic for backcountry use in the United States of America. Although minimum impact programs such as LNT were begun in the early 1970s, they floundered until the National Outdoor Leadership School (NOLS) became a founding member in 1991.[60] Today, LNT and minimum impact camping are essentially synonymous terms and are the basic standard against which backcountry users are measured. The main reason for these minimum impact camping techniques was that they "allowed an ever-growing number of backpackers to visit wilderness, while leaving its ecological integrity intact."[61] This comment reflects what may be another reason for the scarcity of ecological knowledge in OR: the reason for adopting these practices had to do less with understanding and protecting the ecological conditions of an area than with getting more people into those areas without feeling guilty about destroying them.

---

[57] Simmons (1985).

[58] For example, the Boy Scout movement, the YMCA, the anti-modern and woodcraft movements, and various nature writers have all addressed early aspects of outdoor recreation.

[59] Turner (2002), 468.

[60] Reed (1999).

Statements in the literature, such as the ones above, lead me to hypothesize that OR would make use of ecological knowledge in determining the seriousness of the impact users were having and in exploring possible solutions to mitigate these impacts. Instead, by the early 1990s minimum impact practices were adopted as the standard for all backcountry travel in the USA. This solution did not require detailed analysis of ecological concepts: if users could minimize their effects to the point where they were nearly invisible, then there was no need to understand the complex workings of nature. If we were not there, we would not damage it.[62] In the absence of detailed ecological knowledge, OR began searching for the mythic zero point where users would leave absolutely no trace of their passing.[63]

---

[61] Turner (2002), 463. See also Manning (1999). As an illustration of the degree of changes LNT inaugurated, Roggenbuck (2000) claimed that "[i]n the name of Leave No Trace (LNT), outfitters will put down carpets in wilderness to catch crumbs during dinner and protect feet from biting ants. Wilderness visitors will minimize the size of campsites and remove fire rings rather than 'improve' the sites through human ingenuity. The art of building a cook fire may be lost, giving way to the ecological lessons of what critters live in and under firewood, what wood burns best, and how ancestors and pioneers lived in nature. Adventure schools will teach greater and greater numbers of people to live comfortably in the woods. Nature stores will sell food containers so that bears and lions don't rob people in the night. They might even sell bear-proof containers to sleep in so that visitors will be safe!", 16.

[62] Perhaps the idea that all effects could be eliminated explains why "[o]utdoor recreationists … rarely see themselves as degrading or affecting natural environments" (Camp and Knight 1998, 892). If impacts can be eliminated, it is not surprising that OR has not taken seriously the insights from ecology.

[63] In a study that sampled company policies on minimum impact, a common type of reply included comments such as "[i]t is our responsibility to ensure that no evidence of our visits will remain" (Blangy and Nielson 1993, 359).

The history of camping and outdoor recreation in Canada parallels much of the history of camping in the USA.[64] After World War II, camping numbers also increased dramatically in Canada. National and provincial parks continued to provide most of the camping opportunities. Supply, however, did not meet demand. In 1968 one Ontario park director commented that "[w]e are already in danger of destroying the natural beauty of some of our parks with the number of campsites we are allowing. And yet, we are still not coming close to satisfying demand."[65] In terms of protected areas, it was recognized as early as 1970 that

> ecological considerations had almost no part in the establishment or design of any of the Canadian national parks.... It is equally safe to say that most of the ills that beset our national parks have an ecological component and arise from proceeding in the absence of policy objectives framed in ecological terms, and from decisions made in ignorance of ecological alternatives and consequences.[66]

If the design and management of national parks proceeded without detailed ecological knowledge, it should not be surprising that the field of OR reflected that absence. Thus, the recognition that increasing OR demand threatened the integrity of natural areas grew from the late 1960s onward, but the ecological knowledge needed to grasp the complexities of the situation, although present to an ever greater degree in ecology, was absent in OR.

In a way, this absence did make sense. At first it may seem that ecology and environmentalism would be closely linked. If so, then as pressures on natural environments increased and environmental concerns arose in the 1960s, one would expect to see ecology being recruited more often in the service of environmental causes, including the protection of wilderness and natural areas frequented by outdoor users. As Bowler and Morus noted, though,

---

[64] For more information on the history of outdoor recreation and camping in Canada see Butler (1989), Marsh and Wall (1982), Morrison (1982), Page (2000), Wall (1989), and Wall and Wallis (1982).

[65] Wall and Wallis (1982), 344.

[66] Cowan (1970), 321. Sadly, over twenty-five years later, according to the *State of the Parks* report in 1997, the ecological integrity of most of our national parks is even more seriously threatened. For example, twenty-two parks out of thirty-five (for which data were available) received the worst or second worst rating on ecological integrity Gordon Writing Group (1997). Accessed March 14, 2008.

> [a]lthough many people associate the term 'ecology' with the environmentalist movement, we have seen that scientific ecology has a variety of origins, most of which were not linked to the defense of the natural environment. Science has more often been associated with efforts to exploit natural resources, and historical studies show that ecology emerged more from a desire to manage that process than to block it.[67]

It makes sense, then, that in terms of the concerns over environmental damage that were arising in outdoor recreation circles, ecology would not necessarily have been a solid ally in the quest to stop this damage. The lack of support ecology lent to these efforts can be seen more specifically in the attitudes toward wildlife preservation,

> which are routinely justified in terms of ecology and evolution even though (1) [attitudes toward wildlife preservation] developed in terms of a pre-evolutionary and ecological conception of species as fixed and immutable and (2) were little affected by the new ecological and evolutionary perspectives that replaced that conception. Even today, in fact, the practical influence of the theory of evolution and the science of ecology on our behavior appears to be marginal at best.[68]

To the extent that outdoor recreation, in the 1960s, saw itself as championing environmental issues such as wildlife preservation, it joined ranks with the environmental movement in general in adopting models and theories reminiscent of nineteenth-century attitudes about nature. Attitudes toward wildlife preservation changed but little with the advent of newer ecological models that challenged the nineteenth-century belief in species as fixed and immutable.[69] OR (along with the environmental movement) often remained committed to these older ideas.

---

[67] Bowler and Morus (2005), 233. Perhaps because science often serve the interests of industry, "[p]resent recreational and other land-use mapping, zoning, and decision-making appear to take insufficient account of ecological or historical geographic aspects of landscapes" (Nelson 1970, 12).
[68] Hargrove (1989), 129.

The incompatibility between outdoor recreation (over)use and ecological biodiversity was becoming more and more obvious beginning in the 1970s. By the late 1990s, the role and importance of biodiversity had grown in Canadian society to become one of the top political issues. The public was demanding the preservation, protection, and restoration of biodiversity in Canada. Public understandings of biodiversity have been "shaped by conceptual models and values that help people make sense of their relationship to nature. These models and values influence human attitudes and behaviour (including recreational activities and purchasing and voting behaviour) that affect biodiversity."[70] For those members of the Canadian public who were backcountry users, the value that wilderness had as a place without evidence of humans where natural processes could continue unimpeded was being threatened by the ever-growing numbers of users. The concern that the Canadian public expressed in terms of the health of the environment is similar to the concern that OR elicits from participants and researchers. Backcountry users are concerned with the changes that wilderness areas are undergoing across the country. Moreover, they are concerned not just with having places to roam freely, but also with having a healthy level of biodiversity in Canada. The concern for biodiversity has only increased over the past four decades and provides another link between OR and the environmental movement. A focus on the science behind the environmental issues shows that OR is connected more closely to the environmental movement's emphasis on broad-reaching, abstract, and somewhat amorphous concepts

---

[69] For discussions on the development of wildlife conservation in Canada, see Foster (1978) and Loo (2006).

[70] Vanderlinden and Eyles (2000), 237.

(such as biodiversity and wildlife preservation) than to the ecological science that studies these issues.

In the outdoor recreation literature on Canada, like its counterpart for the USA, ecology as a discipline is mobilized in efforts to protect ecosystems, biodiversity, and wilderness quality in general. References to the specifics of ecological science in outdoor recreation literature are scant. Instead, OR focused on issues such as crowding, satisfaction, constraints, motivation, and benefits. Furthermore, as Hargrove suggested, ecological concepts in OR literature often harkened back to old and outdated models. There can be no doubt of the genuine concern for the natural environment in OR. Regrettably, this does not appear to be as grounded in current ecological knowledge as could be. If ecology does not form the foundation for minimum impact practices as much as expected, then on what are these practices grounded? How accurately is OR able to assess its impacts, and what kinds of strategies for solving these problems are on hand when ecology is largely written out of the equation? What would happen if the OR literature were to take ecological theory into consideration? Moreover, what would happen if OR began to take more recent ecological theory into consideration? Ecology has changed considerably since the first ecological society was founded in 1913 in Great Britain.[71] The last forty years have seen the most significant changes in ecological theory in terms of relevance for OR practices. These changes occurred at the same time as OR was developing its commitment to minimum impact research and articulating the connection between broad-reaching concepts such as biodiversity, ecosystem integrity, and the health of wilderness areas in Canada. And yet relatively little of this ecological modelling made its way into OR.

---

[71] Golley (1993), 9.

Chapter Outline

Chapter Two of this dissertation begins with a discussion of the power/knowledge regime in modernity. I begin here because ecology is a science that arose in the middle of the 1800s as part of the wider development of the sciences. Ecology is actually a relative latecomer in this development. For example, the first use of the term 'ecology' occurred in 1866, when Ernst Haeckle used it, whereas the appearance of the terms 'biology' and 'physics' occurred significantly earlier. This time-line positions ecology well within modern society. Consequently, Chapter Two examines the contours of modernity in terms of games of truth and regimes of discipline in order to place ecology within the structure of modernity. To understand how this context influenced the discipline of ecology, I use an archaeological perspective to examine ecological discourse. That is, I parse ecological discourse according to disciplinary functions and rules of formation. I use a thematic organizing principle to structure the chapter. The relevant themes in modernity that both constrain and enable ecological discourse are progress, development, and stability; mechanisms of universalization and standardization; and a disciplining gaze that penetrates the surface to reach a deeper structural level. Rather than focus on specific developments or scientists in ecology, I show how the production of ecological knowledge in each theme is organized according to certain principles and rules.

Chapter Three examines outdoor recreation discourse, specifically that of minimum impact, and the goals of management related to ecological integrity. OR is also produced within modernity; however, the influence of modernity on it is not identical with its influence on ecological discourse. I employ both archaeological and genealogical perspectives in this chapter. In Chapter Two, I am interested in how ecological discourse functions. Chapter Three has a similar goal, but also adds a genealogical purpose: I want to underscore the development of the modern subject in OR discourse as it relates to modern society more generally. In other words, while OR produces knowledge according to certain rules and procedures, it also produces and disciplines a certain kind of subject. Various disciplinary techniques become salient at this point. Thus, Chapter Three focuses on surveillance, normalization, standardization, authenticity, and the atomic self in relation to the ways in which subjects are produced, positioned, and empowered in OR discourse. This chapter engages archaeology in terms of how OR discourse operates, but it also engages with the ways that power/knowledge constructs and empowers subjects of a certain type.

Chapter Four looks at the main consequence of how both ecology and OR discourse function and the subject that is implicated in OR. In modernity the human subject has been constructed as separate from nature. This chapter begins with a brief history of this separation and takes the Enlightenment as its starting point. Ecology and OR can be seen as an outgrowth of the Enlightenment and share some of the discursive practices that developed during that time. For example, the main practice they share is a conceptual and physical separation of humans from nature. This separation has consequences. For example, even when, on the surface, the discourse speaks about natural nature,[72] there are many instances when the focus reverts to human-centred issues.

---

[72] This somewhat awkward term is used to emphasize the point that ecologists and OR scholars most often study a particular form of nature that bears little or no evidence of human interaction or alteration. Nature, under this conceptualization, is "any thing, process, or event, or any aspect of a thing, process, or event, that exists, happens, or changes not as a result of human activity; in other words, nature includes that which is not under the control of, or shaped by, human activity" (Schatzki 2003, 85). Natural nature is not nature in the sense of a lawn; it is nature in the sense of a pristine meadow. As Worster (1995a) defined it, "[b]y common understanding we mean by 'nature' the nonhuman world, the world we have not in any primary sense created. The 'social environment,' the sense of humans interacting only with each other in the absence of nature, is therefore excluded. Likewise is the built or artifactual environment, the cluster of

This shifting occurs because, even though both disciplines claim to draw a sharp distinction between humans and nature, the distinction cannot be clearly maintained. Scare quotes are another effect and indicator of the human/nature split. Authors in both fields often use single quotation marks to set off problematic terms that are vague and/or unclear in their meaning. It is telling that the words most commonly treated in this fashion are the cornerstones of the discourses. That is, *nature*, *pristine*, *wilderness*, *authentic*, *natural*, and the like are placed in scare quotes. Although each discipline depends on the existence of natural nature, it seems that neither can clearly define it. Finally, Chapter Four shows how each discourse recognizes paradoxes as elements that, while not common, seem to play a problematic role. The exact nature of paradoxes remains unclear in these discourses precisely because of modernity's orientation toward knowledge production. Epistemologically speaking, modern sciences are not paradoxical: factual knowledge, while complex in many instances, remains relatively unambiguous.

Chapter Five delves into the poststructural concepts of the trace and différance, ghosts and spectres, and undecidability to theorize the role and significance of the way in which ecological and outdoor recreation discourses operate to produce and arrange knowledge. Derrida's critique of the metaphysics of presence becomes even more salient in instances where scholars focus on the authentic and true form of nature, natural nature, wilderness, or unspoilt land. Poststructural figures like spectres, différance, and undecidables illustrate how that which is thought to be vanquished, in the case of ecology

---

things that people have made," 19. Wall (1989) also described it as referring "more specifically [to] aspects of nature which are natural in that they have not been modified by human beings," 204. For Miller (1999), wilderness was a place where "nature and natural processes dominate.... Here, in the ideal case, streams flow unimpeded by waterworks. The sounds are of birds, mammals, insects, and flowing waters. There is limited development of roads, buildings, agriculture, and human settlements. While most have felt the hand of human cultures as people have molded and shaped nature over the centuries in their search for sustenance and living space, nature's processes still dominate the landscape," 36.

and OR this is the human presence, returns again to haunt the discourse. This chapter argues that, even though both ecology and OR employ discursive practices that (1) divide and reject humans from nature, (2) produce and depend on a modern conceptualization of the self as independent and objective, (3) rely on modern assumptions concerning the progressive development of knowledge, (4) conceptualize nature through structuralist models that pierce the surfaces to reveal nature's true pattern, and (5) support the idea of an authentic wilderness (natural nature) that needs protection from the damaging effects of human users, neither discourse escapes a paradoxical situation that threatens its discursive formation.

Chapter Six explores the idea of paradoxes more fully to show how they operate and why they are so threatening to modern discourses and power/knowledge regimes. Paradoxes are shown to be neither logical nor illogical, both logical and illogical, and either logical or illogical. This troublesome characteristic of paradoxes helps explain why it is that ecological and OR discourses continually use shifty/ing language and scare quotes and why they remain quiet in reference to paradoxes even as they perform them. Finally, Chapter Seven begins with a discussion of how ecological concepts are used in the discourse of OR. It turns out that, for the most part, the ecological concepts and models that appear in OR discourse are quite dated (in many instances they remain eighty years behind current ecological thinking). Next, I examine newer ecological theory to show how it is inherently and in some cases overtly paradoxical. Chaos theory and landscape ecology are two sub-fields in ecology that display a different orientation toward paradoxes. Chapter Seven concludes with comments on incorporating these models into OR and how both fields might begin to adopt a different epistemological stance from modernity (one that admits of paradoxes). Hence, in Chapter Eight, I conclude with some thoughts on what a deliberately paradoxical ecology and OR might look like and what the benefits would be if this change were adopted.

CHAPTER 2: THE STRUCTURE OF SCIENCE AND ECOLOGY

Knowledge itself is power. ~ Francis Bacon, Of Heresies

In this chapter, I explore the relationship between the power/knowledge nexus of the modern disciplinary regime and the field of ecology. Because ecology forms the stated rationale for much of the literature in OR, understanding the discursive conditions under which ecology produces its knowledge claims helps us to compare and contrast ecology with OR. Archaeology is useful in this task because it shows how the current discursive formation of ecology is informed by historical conditions and techniques. The same is true for OR, although the historical particulars are somewhat different. I contend that, in looking largely within disciplinary boundaries as opposed to across them, OR scholars and ecologists have been less able to see how other disciplines intersect and influence their own fields. Furthermore, the discursive practices in ecology and OR combine with the modern disciplinary regime to produce a particular type of subject who views the natural world. Thus, I use genealogy to explore the connections of descent that link the production of the modern scientific subject with the subject who uses (and damages) wilderness ecosystems. From there, I offer alternative interpretations of our impacts on the natural world.

Historical conditions gave rise to the modern techniques of the power/knowledge regime that are themselves a reflection of what Foucault called the will to know. Science, as a premier modern accomplishment, exemplifies many of these techniques. Both ecology and OR express this will to know in their production of and reliance upon a narrative of the progressive, refined development of factual knowledge about nature. Questions central to my investigation include What is the ideal for how modern science should work?; What kinds of conditions, constraints, and opportunities does that ideal place on knowledge?; How do ecology and OR employ select techniques of the disciplinary regime to construct their objects of knowledge?; and By simply using those techniques, how do ecology and OR further disseminate them?

Increasing our understanding of the organising practices that have guided and shaped the construction of knowledge in ecology moves us closer toward comprehending OR's discursive practices. Because both fields are heavily influenced by the discursive practices of science, we can understand how one field's practices are implicated in other field. Teasing apart the rules and tactics of production from the actual knowledge produced is a complex and subtle project. I begin to accomplish this by using select examples of objects produced by ecology (such as a particular model of the ecosystem) to illustrate the requirements to which such objects adhere. Then, in Chapter Three, I take select objects in OR discourse (such as the idea of wilderness as an authentic pristine place) to illustrate how they are produced by the same discursive practices I outlined for ecology in Chapter Two. My objective for the next two chapters is to make apparent the connections between the production rules in ecology and OR. Although there are differences that will be noted, science in general provides many of the connections between ecology and OR. In this present chapter, then, I illustrate the consistency in the discursive practices of ecology. As a discipline, ecology has remained remarkably consistent in the way it produces knowledge. This consistency is related to the consistency in the discursive practices of science in general. Only recently did shifts in science in general begin to affect the discursive practice of ecology. These shifts mark the beginning of a new discursive regime, if you will, that provides me with the platform I use in Chapter Seven to mount interesting and challenging questions about OR discourse.

I take Foucault's games of truth (that is, the will to truth and the will to know) as the point of departure into the modern disciplinary regime. These wills are evident in modern science. For our purposes, the relevant techniques of the modern disciplinary regime are surveillance, normalization and standardization, and differentiation and classification. We can understand how these techniques are employed by science in the service of these wills; in other words, what is often held as the ideal model for science reflects the structure of the modern power/knowledge nexus. After describing this model, the second section of this chapter shows how ecology is heavily indebted to the discursive practices of ideal science. This second section looks at the structure of ecological discourse to determine how power/knowledge and the disciplinary regime manifested in it from 1913 to the 1970s. As we will see, the rules of this period are quite consistent with OR's discursive practices. It was only after the 1970s that a shift in the rules of production in ecology occurred for which there appears no analogous shift in OR.

Because discursive practices and rules of production are central concepts in this chapter and the next, a few explanatory words about them are necessary. As Foucault said "[d]iscursive practices are characterized by the demarcation of a field of objects, by the definition of a legitimate perspective for a subject of knowledge, by the setting of norms for elaborating concepts and theories. Hence, each one of them presupposes a play of prescriptions that govern exclusions and selections."[1] In other words, discursive practices are the rules that govern the field of ecology as well as give that field its legitimacy (that is, within which legitimate speakers produce legitimate and authoritative works). For example, over the first three decades of the 1900s, ecology strove to define the legitimate perspectives as scientific; consequently, it excluded the non-scientific perspectives of natural historians or collectors.

---

[1] Foucault (1997b), 11.

As a result of my emphasis on discursive practices and rules of production, certain developments, publications, and scientists appear more prominently here than they would in a narrative or biographical history. Vastly different developments have occurred in ecology throughout the twentieth century; nevertheless, most are enabled and constrained by the formations and practices outlined here. Of course, there are some points in the history of ecology that do not abide by the schematic of this chapter.[2] The disciplinary structures do not dictate all the form(s) of ecological knowledge. However, the vast majority of examples of ecological knowledge production do, at least until the 1970s, conform in large measure to the discursive structure I outline in this chapter. After the 1970s, though, the discursive structure of ecology begins to shift. From the early 1980s on, new objects were formed and arranged in different relationships with the older ecological objects. Although it is the case that new objects and relations were forged during this period, the rules governing their production changed less dramatically. In fact, it is more accurate to say that post-1970s ecology reflects a *suggestion* for change to the rules of production rather than an *actual* change to those rules. Even though the rules of production remained relatively unchanged, there were significant changes in the objects, concepts, and statements during this time. Changes at the level of the objects, concepts, and elements produced by ecological discourse are not merely surface-level change; they also point to the deeper, more profound structural change I outline in Chapters Six and Seven.

---

[2] For example, Leopold and Jones (1947) wrote an article that was highly descriptive and based on careful observation instead of measurement, manipulation, and examination. The authors included a fifteen-page table containing 328 entries that recorded events such as the dates of flowering for plants. In addition to the emphasis on observation and record keeping, Leopold and Jones suggested that "[o]nce he learns the sequence of events, the phenologist falls easily into the not-very-objective role of successful seer and prophet. He may even fall in love with the plants and animals which so regularly fulfil his predictions," 203. This methodology bears striking similarity to earlier methods of walking, waiting, and watching that natural historians used. It also stands in contrast to the objective and removed type of study that twentieth-century ecologists were using more and more often.

Games of Truth, Regimes of Discipline

You put your will and your values upon the river of becoming; what the people believe to be good and evil betrays to me an ancient will to power.
~ Friedrich Nietzsche[3]

Foucault characterized much of modernity as a quest for knowledge. Following Nietzsche's phrase, 'the will to power,' he called this the will to know. While seeking knowledge was certainly not a new development in modernity, there are significant differences between this will to know and the older will to truth that Foucault defined as the ethical caring for oneself. The will to know took the form 'know thyself' and had its base in factual knowledge generated by rational enquiry. Foucault suggests that, from questions of 'What is the correct treatment of myself?,' 'How can I rid myself of bad habits?,' and 'What does it mean for me to live a good life?,' we have turned to questions of knowledge about the self: 'What am I?,' 'What is normal?,' and 'What should I be?'. These newer questions required knowledge and training of a different sort, which the modern disciplines could provide. Psychiatry, for example, tells us what is wrong with us, but, at the same time, defines what it is to be normal.[4]

---

[3] Nietzsche (1987 [1885]), 136.

[4] See Foucault (1997b), Joseph (2001), and Rabinow (1997) for more on the will to truth and the will to know. In earlier works, Foucault saw the will to know as a particular manifestation of the will to truth. Later, he saw it as eclipsing the older will to truth. I am following his later formulation even as I draw on his earlier works in some instances. See, also, Nietzsche (1967 [1901]) who saw the "[w]ill to truth … *as the impotence of the will to create*" (317).

We can think of the will to know and the will to truth as different games. "The word 'game' can lead you astray: when I say 'game,' I mean a set of rules by which truth is produced. It is not a game in the sense of an amusement; it is a set of procedures that lead to a certain result, which, on the basis of its principles and rules of procedure, may be considered valid or invalid, winning or losing."[5] The procedures that fields such as psychiatry, biology, and medicine use to produce the truth form a part of the disciplinary regime. In games of truth, the tactics and procedures are subtle and pervasive; they discipline the subject by investing him or marking her as a legible surface upon which the norms of disciplinary power can be etched. In other words, we become (or not) good citizens, efficient students, effective soldiers, and healthy humans by internalizing and striving to obey the discursive norms of modernity. This, of course, is never a completed project; it is always a continuing struggle whereby individuals can resist the pressures of disciplinary power. We are subjected to these pressures, certainly, but, at the same time, we have the power to resist these norms. If resistance was impossible, it would be pointless to challenge the ways modern power builds and positions subjects because there would be no alternative, no way out, and no place for resistance. The potential for resistance is, in fact, the condition of possibility for my suggestions of change in later chapters.

Still, we need to recognize a significant shift in the operation of power/knowledge and discipline from earlier forms of spectacle (for example, public executions and torture) to its modern manifestation in sciences like ecology. A major distinction, then, in the will to know is the advent of disciplinary techniques that "attained a level at which the formation of knowledge and the increase of power regularly reinforce one another in a circular process."[6] The techniques of disciplinary power can be summarized as procedures for

---

[5] Foucault (1997a), 297. See also Faubion (1998), xxv.

> distributing individuals, fixing them in space, classifying them, extracting from them the maximum in time and forces, training their bodies, coding their continuous behaviour, maintaining them in perfect visibility, forming around them an apparatus of observation, registration and recording, constituting on them a body of knowledge that is accumulated and centralized.[7]

These techniques reinforce each other such that knowledge gains the power to craft and shape the individuals who produce knowledge. Certainly, as Foucault points out regarding the prison system, the older modality of power also marked bodies (as in torture and hangings). However, the modern manifestation of power couples it with knowledge and spreads this power/knowledge nexus through new and profuse channels, such as the sciences. With modernity, the sciences became a powerful vehicle for understanding people, distinguishing right from wrong, and separating normal from abnormal. Science was also used to increase the usefulness of bodies. Understanding human beings (as in human biology and psychiatry) functioned in part as a means of extracting labour and, more generally, of influencing behaviour.

A number of techniques and tactics are used to code behaviour, fix individuals in space, maintain bodies in near perfect visibility, and catalogue the types of people. In terms of modern ecological science, it is fruitful to examine some specific tactics: (1) surveillance, (2) normalization and standardization, and (3) differentiation and classification. These tactics occur in a broader scope than just the sciences, but, as I demonstrate, they are central to the scientific endeavour. The example of surveillance will serve to illustrate this broader scope. The potential for surveillance in modern society is ubiquitous; it is present in traffic and store cameras, the design of our schools such that each student has a clearly marked place, hospitals and prisons that keep a close eye on patients and inmates, and modern warfare's use of satellite technologies.[8] By placing the modern subject in the context of perpetual potential surveillance, the disciplines more clearly constitute the norms for society. According to Foucault, surveillance, and the accompanying normalization, "becomes one of the great instruments of power at the end of the classical age."[9] For example, normalization helped establish the principle of standardization in teaching (including standardized curricula and exams), a standard national medical profession, standardization in the industrial process, and, as we shall see, standardized practices for ecologists and backcountry users in Canada.

---

[6] Foucault (1995), 224.
[7] Foucault (1995), 231.

The disciplinary techniques of surveillance and the paired normalization and standardization work to individualize the modern subject: "those on whom [discipline] is exercised tend to be more strongly individualized … the child is more individualized than the adult, the patient more than the healthy man, the madman and the delinquent more than the normal and the non-delinquent."[10] Foucault explored the way in which the human sciences manifest the disciplinary regime. According to him,

> [t]he moment that saw the transformation from historico-ritual mechanisms for the formation of individuality to the scientifico-disciplinary mechanisms, when the normal took over from the ancestral, and measurement from status, thus substituting for the individuality of the memorable that of the calculable man, that moment when the sciences of man became possible is a moment when a new technology of power and a new political anatomy of the body were implemented.[11]

---

[8] See Ryan (2005) for a longer discussion of surveillance, outdoor recreation, and war.
[9] Foucault (1995), 184.
[10] Foucault (1995), 193.
[11] Foucault (1995), 193.

In these scientifico-disciplinary mechanisms, subjects are individualized by the techniques of normalization and standardization. The more knowledge that could be gained about a person, the more that individual could be compared to the norms and standards. Individuals are also compared to one another and classified according to norms and standards (for example, some students pass the grade while others do not). By instituting procedures of partitioning, the sciences introduce, between different elements at the same level, solid separations and thereby define compact hierarchical networks.

One of the results of the scientific techniques designed to identify and classify is the production of factual knowledge. As the ecologist Lindeman stated, the bottom line for "scientists ... is to 'get at' reality."[12] For ecologists and OR scholars, this reality is mainly a natural one. The techniques of the will to know ordered and classified various forms of knowledge about nature along a spectrum that extended from the most reliable, valid,[13] and objective to the most subjective and creative. At the subjective end of this spectrum lies art and poetry, at the other end are the 'hard' sciences (such as physics and chemistry) that deal with matter, mathematical concepts, and principles. [14] The factual knowledge emphasized by the tactics of power/knowledge falls closer to the objective and rational end of this spectrum. Foucault suggests that the rejection of subjective knowledge that accompanies this type of spectrum constitutes one of the three great systems of exclusion governing discourse.[15] Ecologists, for instance, sanctioned against the use of concepts and terms linked with subjective and qualitative perspectives. They privileged order over chaos, which is reflected in their emphasis on rationality over irrationality. In OR, the same division (between objective and subjective knowledge) operates but has a different result. In ecology the emphasis on objective knowledge was directed toward understanding the natural world. Much of the literature in OR, on the other hand, focuses on factual knowledge regarding the subjective and experiential elements of wilderness use. This focus on factual knowledge helps explain how ecology figures in OR literature. In each discipline, researchers emphasized factual knowledge, but the objects of that knowledge differed. In ecology, knowledge is valued when it increases our understanding of the world around us; whereas, in OR, ecological knowledge is important mainly because it aides us in understanding users' subjective experiences in the wilderness.

---

[12] Lindeman (1940), 367.

[13] I am using the concept of reliability to refer to the consistency of results from the same test repeated two or more times. The more the results recur in multiple re-tests, the more reliable the test is. Validity, on the other hand, refers to the accuracy of an instrument: does the model measure what it purports?

[14] See Odum (1983), iv who contrasts the hard sciences like mathematics and physics with the softer human ones. Ecology, he feels, lies in between.

[15] Foucault (1972b), 219.

By rejecting some types of knowledge in favour of another, ecology embodied one of the main themes in science: progressive development. Reliable, valid, and objective knowledge were seen as a sign of progressive development. When we assume, in general, that our understandings of the natural world become more refined and accurate over time, we closely link truth with progress and development. The division of truth from falsity is one of the hallmarks of the modern power/knowledge regime and is a central element in modern science. Without the notion of truth, scientists would not be able to distinguish their work from fantasy. Science is at base the quest for truth; it does not purport to create myths or fantasy. The ideal of progressively developmental knowledge manifested in claims that over time ecology 'advanced' closer and closer to understanding the truth about the mysteries of nature.

The will to know, as exercised by ecology, divided knowledge not only into factual and fictional parts but also into compartments. Just as disciplinary techniques divided students into different grades and one kind of patient from another, so too did ecology divide nature into sections. So, for instance, in the early 1900s Professor Drude proposed eighteen climatic types, arranged under four groups. He further classified plants as falling into one of the following categories elementary associations, associations, formations, formation-groups, and vegetation-types. These compartments were an attempt at defining the units of nature and showing how they related to one another. In a similar project, the search for truth led ecological science to probe deeper and deeper into nature to unlock its secrets. Ecologists wanted to know what made nature work. What lay hidden under its surface? The deeper ecologists looked, the more ecology was thought to be progressing toward the truth of nature.

This chapter is comprised of three sub-sections: the epistemological figures of progress, development, and stability; the mechanisms of universalization and standardization; and the structuralist reliance on a deep penetrating gaze. The conditions and processes for knowledge production (that is, the rules for discursive practice) reflected in these sub-sections are (1) the various rituals and prohibitions surrounding and constructing legitimate forms of knowledge; (2) a division and rejection of knowledge in the form of great systems of classification; (3) the privileged right of the objective, rational, and independent subject; and (4) the fellowship of scientific discourses that constrains and enables knowledge production. Each sub-section shows how various discursive rules manifest in different themes in ecological discourse.

The Disciplinary Structure of Ecology

As natural selection works solely by and for the good of each being,
all corporeal and mental endowments will tend to progress toward perfection.
~ Charles Darwin, 'On the Origin of Species'

Ultimately, man finds in things nothing but what he himself has imported into them:
the finding is called science, the importing—art, religion, love, pride
~ Friedrich Nietzsche[16]

As part of the larger scientific project, ecology expressed a growing desire to move away from earlier forms of natural history and produced a certain kind of knowledge that was now called data. Ecologists sought data that they regarded as quantitative as opposed to qualitative, and objective rather than subjective. These data emphasized prediction and control over description and taxonomic classification. The development of ecology from natural history was a process in which many of the characteristics of the scientific endeavour came to the forefront. This larger, ongoing endeavour formed the general surface of emergence upon which more specific examples of narratives of progressive development, universal categories and models, and the deep penetrating vision of science can be set.[17] I begin this section, then, with a discussion of the development of ecology from natural history in order to better situate the techniques that ecology used to organise and produce knowledge.

The first professional journal devoted strictly to ecology, *The Journal of Ecology*, debuted in Great Britain in 1913. From its inception, ecologists bent their efforts toward establishing their discipline as a science. Arguments in ecology were often directed against natural history and natural collectors.[18] By the time *The Journal of Ecology* began, many ecologists had already begun to move away from natural history's focus on collections and labels. These ecologists pushed for a more scientific approach to the study of nature. In 1914 Dachnowski advocated for an international organisation of ecologists, because this would permit

---

[16] Nietzsche (1967 [1901]), 327.

[17] Foucault (1972a) uses the concept of surfaces of emergence to refer to the matrix that makes the arrangement of discursive objects and their relations with each other legible and comprehensible.

> [t]he investigation of problems which cannot be solved unless observations and experiments are made under standard conditions accepted by common agreement, and with appropriate methods of ecological study, instruments and units of vegetation (nomenclature, specification, etc.), yielding comparative data and records.[19]

Dachnowski's wish marks the continuing entry of scientific thinking into ecology. This entrance was a process rather than a point and had begun in other natural sciences, such as biology, over fifty years earlier with Darwin's work, *On the Origins of Species* (1859). As a younger science, ecology did not begin to advocate for this more scientific perspective until later.

The distinction ecologists were making can be briefly examined using the case of the cabinet of curiosities, which was a phenomenon more representative of natural history than of ecology. The cabinet of curiosities contained and displayed the untamed element of nature. It did not transform nature; it retained (but also contained) nature's wildness. In these displays, individual elements like tusks, tigers, and tribes were the wild, natural aspect being contained and displayed. Each artefact, humans included, provided its own context and, thus, could "easily be collected and moved for exhibition in cabinets on the other side of the world."[20] These cabinets and exhibitions have been discussed in the context of colonial expansion whereby people and objects were captured and shipped back to England as evidence of the success of colonization and empire-building. By the time the *Journal of Ecology* began, ecologists were focusing less on displaying individual elements in self-contained dioramas or cabinets and more on systematic and comprehensive classifying schemata.[21]

---

[18] This, however, was not a uniform development. As late as the 1960s, some ecologists studying plankton were still conducting the kind of descriptive studies criticized early in the twentieth century (see Minshall, 1988).

[19] Dachnowski (1914), 239.

In Canada, one of the most significant figures in the natural history tradition was Professor John Macoun, a member of the Canadian Geological Survey (CGS), botanist with the CGS, then, naturalist for the CGS (which included expanded duties related not only to plants but also to animals), and, finally, in 1887, the Assistant Director of the CGS. He remained in this post until a debilitating stroke in 1912 forced him to retire. His career with the CGS lasted for 31 years, during which he collected over 100,000 specimens from all over Canada.[22] Macoun's type of study differed significantly from the practise of his contemporaries. When ecologists were calling for more scientific approaches, Macoun argued the opposite. He lamented the lack of observational skills in most of the newly trained scientists. He felt that learning to look and see was the most important ability in a natural historian.

Early ecologists, on the other hand, felt analysis was more important than displaying specimens in museums. These ecologists emphasized experimentation and sought to produce results that could be compared and built upon by others. For Macoun, studying nature was one way of coming to appreciate the beauty and majesty of God, who created nature in all its splendour. By the turn of the century, ecologists were distancing themselves not just from exhibitions but also from all non-scientific projects. For the first half of the twentieth century they sought to 'scientize' their field by consolidating, standardizing, and modernizing it. This desire for scientific status manifested as an aversion to qualitative data. In 1935, for instance, de Peralta felt a "great need … of replacing qualitative knowledge of competition by experimental, quantitative data."[23] Even by 1946, when Clarke surveyed the state of research on marine productivity, he still noted with dismay that quantitative research had been especially sparse.[24] Quantitative data were seen as factual, more reliable, and more valid and valuable, and more and more ecologists were conducting this type of research. When Park reviewed the history of population ecology in 1946 he noted that "growth of the subject has probably been catalyzed all along the way by a feeling on the part of researchers that the phenomena of synecology (group ecology) should be expressed as quantitatively as possible."[25] Through this allegiance with quantitative data, ecology sought to purchase itself a better standing in the scientific community. These early ecologists were arguing, in effect, that quantitative data were more scientific because they were more objective and offered better predictive possibilities. Ecologists were becoming interested in prediction instead of just description, and quantitative data were more conducive in this endeavour. In this manner, ecology played its part in furthering science's emphasis on facts, truth, and progress.

---

[20] Broch-Due (2000), 17.

[21] For more information on the cabinet of curiosities, see Bennett (1994) and Gyan (1999).

[22] See Waiser (1989) for a biography of Macoun and his influence.

The switch from qualitative to quantitative studies was not quick; it spanned a number of decades. Even though ecologists around the turn of the century had begun thinking qualitatively about systems, it was not until half a century later that they "began to develop the definitive, quantitative field of ecosystems ecology."[26] By the 1950s, nearly all ecological analysis strove for this type of definitive knowledge. One might begin with a descriptive overview of the issue, concept, or area, but more and more often ecologists were, in the end, seeking factual answers. Recounting the history of an area for the past 5,000, 10,000, 5 million, or 1 billion years helped to situate the study, but the real aim was to provide detailed quantitative information that could be used for prediction or control.[27] For example, in 1955 the Odum brothers' ecological analysis built to a point where "[f]inally, the quantitative trophic structure of the reef community can be set out."[28] The marshalling of information, descriptions, and facts ultimately lead to the construction of quantitative models that could be used to make predictions.

---

[23] de Peralta (1935), 357. See also Tansley (1939) who privileges quantitative over qualitative data.
[24] Clarke (1946).
[25] Park (1946), 318. See also Thornthwaite (1931) who ended by noting that he hoped "that other geographers may be inspired to make further studies of climate on a quantitative basis," 655.

By the 1960s, quantitative analysis was firmly positioned as the dominant form of ecological research. As the quantitative approach gained in popularity over time it became less and less necessary to emphasize the distinction between qualitative and quantitative data. Ecologists were able to state simply that "[t]he objective of this investigation was to obtain a detailed quantitative analysis of the dynamics of a natural animal population."[29] It was still necessary to highlight the type of data, but the desire to place as much distance as possible between the better, more valid, more useful quantitative studies and the less scientific qualitative studies had diminished. Baydack summarized this emphasis on hard, quantitative data: "science does not consider values, but facts: organized, tested, and accepted knowledge."[30] Ellison also commented that "[t]esting is the essence of modern science: no matter how fascinating a theory may be, it must be weighted against reality, the ultimate criterion."[31]

---

[26] Odum (1983), 14 (his emphasis).

[27] For examples of ecology's use of big history see Braun (1947), Brown (1941), Clark (1990), Cooper (1960), Lichter (1998), Liu (1990), Mayle and Cwynar (1995), Thornthwaite (1940), and Wells (1962). For a counterpoint, and one of the lone voices that critiques this kind of homogenous big history, see Cain (1947).

[28] Odum and Odum (1955), 310.

[29] Cooper (1965), 377.

This desire to emulate scientific standards is one example of the discursive practice that governed much of the ecological discourse. Studies were organized according to the scientific method, and models were designed to yield predictions, which were then tested against reality. Furthermore, the aspiration to science meant that the objects of ecological discourse were thought of, discussed, organized, dissected, treated, classified, and described according to the principles of scientific analysis. For the most part, this practice meant that for the first half of the twentieth century nature was seen almost exclusively as a machine with interdependent working parts. Models and theories in ecology were designed to test this idea. Nature was treated as if it was a machine and in the process the knowledge gained supported the mechanistic conceptualization of nature. The other significant discursive practice involved seeing nature as an organism; however, this was less a holistic organic view than might be expected. In keeping with the human sciences that analyzed bodies in terms of processes, functions, and compartments, ecology began to approach natural systems as organisms in the sense that they could be regarded as organic structures with components that have regulatory functions.

Another method for distinguishing ecology from natural science was the removal of subjective bias. There was a fear of the subjective, as if it tainted ecology somehow. To protect against the damage subjectivity did, the argument went, it must be removed altogether. This removal represents a twist on the debate between qualitative and quantitative data. Whereas qualitative data most often came in the descriptive or taxonomic form, they were, nonetheless, supposed to correspond with reality; subjective data, on the other hand, are not necessarily consistent with reality. They arise from the individual ecologist's beliefs, feelings, and perspectives. By the 1990s, the emphasis had shifted from proving a study was quantitative instead of qualitative to protecting ecology from bias by proving it was objective.[32]

---

[30] Baydack (2000), 181.
[31] Ellison (1957), 63.

When ecologists spoke about standardization, quantitative studies, and objective data, they expressed a desire to attain a certain level of scientificity. Ecologists would argue that "[a]s ecological research progresses and as ecological principles are formulated the study of ecology gradually but steadily emerges from a potpourri towards a coherent and more rigorous science."[33] There is a sense of inevitability: if the scientific method is followed, ecology will gradually develop into a mature, sophisticated, and powerfully predictive discipline. In 1940 Mackaye reflected this desire to attain scientific status. He observed that regional planning, which he linked to ecology, was as old as humanity and yet it had taken this long before "beginning to tackle it in a comprehensive would-be-scientific manner."[34]

These changes in ecology throughout the 1900s are reflective of what Foucault described in *The Archaeology of Knowledge* as the various thresholds of discursive formations. Foucault posited four levels for discursive formations: the threshold of positivity, the threshold of epistemologization, the threshold of scientificity, and the threshold of formalization.[35] These thresholds "mark the degree of systematicity and the objective epistemic authority of any game of truth."[36] The first level is characterized as "a discursive practice [that] achieves individuality and autonomy, the moment, therefore, when a single system for the formation of statements is put into operation."[37] The threshold of epistemologization is marked by attempts (successful or not) at validating norms of verification and coherence, and by exercising a dominant function over knowledge with, for example, key models and critiques. The threshold of scientificity is the moment when the discursive formation not only seeks valid norms of verification, but also obeys certain laws for the construction of propositions, that is, when it obeys certain formal criteria.

---

[32] Other examples of the quest for objective data include Lister and Kay (2000) and Stebbins and Major (1965). Eberhardt and Thomas (1991) give this issue a somewhat different turn. They claim that ecology should be looking for "ultimate understanding," 53. The idea of ultimate understanding coincides well with quantitative and objective data both of which were seen as true forms of knowledge.

[33] Park (1946), 314.

[34] Mackaye (1940), 351.

Finally, there is the threshold of formalization, which is the moment when a scientific discourse is able to define the axioms necessary to it, the propositional structures that are legitimate to it, the elements it uses, the transformations that it accepts, when it can, in other words, take itself as a starting point and deploy the formal edifice that it constitutes. According to Foucault, there is only one science that has succeeded in crossing all four thresholds at once: mathematics. From its beginnings it has followed the same type of axioms and procedures. The same cannot be said for ecology. The analysis presented here will therefore focus on the first three thresholds.

We need to remember Foucault's caution that "each discursive formation does not pass through these different thresholds in turn, as through the natural stages of biological maturation, in which the only variable is the latency period or the length of the intervals."[38] Speaking of the change from natural history to biology, Foucault notes that the thresholds of scientificity and positivity were linked. Biology represents not just a scientific version of natural history; it is its own positivity (discursive formation) as well. In other words, biology should not be seen as merely an outgrowth or maturation of natural history. It is its own separate entity. In contrast, the microbiology of Pasteur

---

[35] Foucault (1972a), Chapter Six, 'Science and Knowledge,' 178-195.
[36] Faubion (1998), xxv.
[37] Foucault (1972a), 186.

> modified the type of scientificity required by … anatomy and physiology, without the discursive formation of clinical medicine, as then established, being made inoperable. Similarly, the new scientificity established in the biological disciplines by evolutionism did not modify the biological positivity that had been defined at the time.[39]

In both these latter cases, Foucault argued, the threshold of scientificity was crossed without a new positivity also being crossed. The change from natural history to ecology/biology represents the development of a new field (ecology/biology) in which the older one (natural history) could not operate. On the other hand, the development of evolutionary thought occurred within the discipline of biology; there was no concomitant appearance of a new field (positivity).

Ecology started to cross the threshold of positivity by placing distance between itself and natural history, by distinguishing itself as a unique domain. The turn of the 1900s represents, loosely, this moment. One of the ways that ecology distinguished itself from natural history was through standardized methods, practices, and instruments that aimed at establishing norms for the verification of knowledge (threshold of epistemologization). Those ecologists that did not follow a quantitative methodology, for example, were engaged in work that in 1946 Park derisively labelled as charming.[40] Ecological models (for example, climax communities, trophic levels, and niche) also played an important role at this threshold. Models were regularly developed to explain the structure and/or function of nature. Through models, much ecological knowledge was verified. These models exercised a dominant function over the creation and verification of knowledge in ecology for the remainder of the 1900s as ecologists fitted data into them, challenged and changed them, or disagreed outright with them; in all cases, the terms of the discussion were set by these models.

---

[38] Foucault (1972a), 187.
[39] Foucault (1972a), 188.
[40] Park (1946), 315.

The models, hypotheses, and theories that ecology developed were themselves designed according to certain formal criteria (threshold of scientificity). In this way, the threshold of scientificity was crossed at the same time, as part of the same process, as the threshold of epistemologization. The crossing of the threshold of scientificity is reflected in the orientation toward knowledge that early ecologists expressed. Whereas natural historians were less concerned with predicting results or understanding the causal connections between elements, ecologists developed their models and theories based on precisely these types of criteria. So an hypothesis in ecology needed to be testable. Testability, in turn, formed one of the main ways ecologists distinguished their projects from those of the natural historians. As a science, ecology sought to understand the structure and function of natural systems and from there learn to predict and control outcomes. In Lutz's view, this "is what may be termed … the seeking of cause and effect relations between environment on the one hand and vegetation on the other."[41] Understanding the causes, functions, and structure of nature, however, was not the final purpose: "[t]he long range objective of the ecologist is to predict, and if possible, to control the quantity and distribution of material in the biosphere, insofar as it is dependent on biological systems."[42] Developing predictive models was one of the things that marked ecology as a science and it continued to be central for throughout the 1900s. In his study of population dynamics in 1965, for example, Cooper used prediction as the basis for defining his understanding of stability: "[i]f stability can be defined as repetition through time such that accurate predictions of future situations can be made, then the pattern of change in size and age structure of the amphipod population in Sugarloaf Lake is highly stable."[43] As ecology's emphasis on prediction grew, it crossed further and further over the threshold of scientificity.[44]

---

[41] Lutz (1957), 46.

The climax community model serves as an example of the norms that ecology adopted for elaborating concepts. A climax community is the end point in the development of a group of species such as in a stand of trees. The climax concept is a long-standing model in ecology and illustrates the centrality of the concept of prediction. Climax models are important precisely because they predict (or claim to predict) the patterns by which vegetation develops: "[t]he concept of climax therefore is fundamental to the prediction of direction of succession, and thus essential as a means of relating widely different types of existing stands into a common pattern of development."[45] Prediction is part of the climax model to such a degree that without it, Whittaker argued, it should not even be considered by ecologists. In fact, in 1953 he said, "it meets a prime test of usefulness of a scientific concept or approach: possibility of prediction. It is suggested that a principal justification for retention of the climax concept is in this possibility."[46] Whittaker's emphasis on prediction as a criterion for the retention of the climax model is representative of many ecological concepts throughout the twentieth century. Models were judged, in large measure, based on their predictive power. Only in the last twenty-five years has this criterion for verification has been challenged.[47]

---

[42] Slobodkin (1954), 83.

[43] Cooper (1965), 390.

[44] See also Watt (1959).

[45] Daubenmire (1952), 306. The concepts of climax and succession are dealt with in detail below. At this point the focus is on the predictive element that appears in various contexts throughout ecology's history.

Closely linked to the idea of prediction is control, another formal criterion for constructing models and critiques in ecology. Prediction enhances control; it does not just build knowledge for knowledge's sake. "Our primary objective," Stark averred, "is to attain a level of knowledge whereby … we will be able to predict the course of populations of the beetle. Such information would be invaluable in control strategies."[48] A model was useful if it could predict outcomes and developments. Prediction, in turn, was useful if it aided in controlling an aspect of nature. Models that accurately predicted outcomes in areas where control was not an issue were not nearly as valuable. Hence, the criterion of control was frequently applied to pests or weeds. Consider Selleck, Coupland, and Frankton who studied leafy spurge in Saskatchewan in the early 1960s: "[l]life history studies … were concerned with various details of developmental history and ecological relations, knowledge of which was expected to aid in efforts to control the weed."[49]

Control not only concerned curbing or stopping outbreaks of insects or weeds. It was also important in increasing the productivity of a forest, farm, or fishery. As a result, in 1950, Puri stated that "[a] precise knowledge of the ecological status of a community … is indispensable … since silvicultural operations … are fundamentally based on accelerating, retarding, or arresting the natural succession of vegetation."[50] In the 1930s and 1940s, these quests for precision drove research into chemical application to crops with the hope of increasing yield.[51] With the advent of more powerful computers in the 1970s, many ecologists felt that "[t]he goals of management, prediction, and control of nature seemed finally to be within reach."[52] Overall, control formed another criterion against which to gauge the usefulness of models.

---

[46] Whittaker (1953), 58.
[47] See also Barton (1993) and Connolly and Roughgarden (1999) for more recent examples.
[48] Stark (1966), 38.
[49] Selleck, Coupland, and Frankton (1962), 27.

In general, ecologists throughout the twentieth century sought data that were reliable, testable, and factual. Models that could be used to predict outcomes and thereby control nature fit this project; however, there were figures besides prediction and control that played an important role in the development of ecological theory. In particular, progress and development assumed an overarching position in ecology. Predictive models were evaluated on their ability to forecast the future, and the future they predicted was invariably a progressive and developmental one that ended in a stable state. Moreover, ecology viewed itself as developmental and progressive. That is, as models and theories changed over time, the discipline advanced.

In order for models to become widespread, terms and concepts had to be standardized and universalized. Idiosyncratic models were of little use to ecologists, as were models that predicted only the behaviour of unique and rare systems. As a result, ecological models mapped out great detailed schemata that predicted the development of vegetation, animal communities, and ecosystems across geographical and geological scales. Hence, in addition to discussing progress and development, I examine universalization and standardization, which are two other discursive elements that drive the production, organization, and dissemination of ecological knowledge. The final discursive element I discuss is the gaze. The attempts to understand the structure and function of nature and to predict and control outcomes were motivated by a desire on the part of ecologists to penetrate deeper and deeper into nature's secrets. As a consequence of this desire, ecology became more and more interested in a structural analysis of ecological systems.[53]

---

[50] Quoted in Daubenmire (1952), 324.
[51] See Fisher, Fults, and Hopp (1946), Jackson (1952), and Pimentel (1966).
[52] Kingsland (1985), 4.

Using Foucault's thresholds to examine the discourse of ecology highlights the rules that govern the production of knowledge in ecology. These rules, in turn, affect outdoor recreation discourse. The research on the impact of OR use, for instance, borrows not only select ecological knowledge, but also the discursive structure of ecology. There is a similarity between the way ecology produces knowledge about nature and OR's understanding of humanity's impacts on nature. For example, predicting impacts and then controlling them is one of the main goals of impact studies in OR. Furthermore, OR assumes a developmental and progressive stance vis-à-vis its understanding of how nature functions and how human interactions change it. The ways in which the power/knowledge nexus interacts with the disciplinary regime in OR is the topic of the next chapter.

---

[53] I want to highlight the tension that lies within the claim that a dispassionate science could be motivated by desire. Furthermore, I am arguing that this tension went unrecognized, even in those ecologists who were motivated by it. This lack of recognition allowed the manifestation of this desire (the desire to penetrate nature's innermost secrets) to occur without comment in the structural models I discuss below. The tension between dispassion and desire becomes even more apparent in these structural models, and I show the analytical opening that derives from recognising how these desires manifest discursively.

Progress, Development, and Stability

Progress, therefore, is not an accident, but a necessity.... It is a part of nature.
~ Herbert Spencer, 'Social Statics'

As ecology crossed further and further over the thresholds of epistemologization and scientificity, it also became more and more embedded in the disciplinary techniques of the power/knowledge nexus. The twin techniques of normalization and standardization, and differentiation and classification are revealed in the epistemological figures of progress, development, and stability. Ecological knowledge that did not conform to these figures was quickly visible as a disruption in the uniformity and orderliness of the models and theories. The debate over the succession/climax model illustrates this process and provides a place to begin.

The history of the climax is often seen as beginning with Clements in 1916 with the publication of his monumental work on plant succession.[54] The concept, however, actually dates back at least to the late 1800s.[55] Clements is seen as the founder of the concept because it was his work, more than any other before, that clearly set out the stages leading up to a climax community. His model was also more precise; it dictated the factors and conditions necessary for a climax community to develop. The climax model became popular and underwent an explosion of terms and subtle variations (for instance, fire-climax, edaphic-climax, and so forth[56]). This expansion and proliferation lasted at least until 1977 when another model was adopted that challenged the prevalence of the Clementsian climax model.

---

[54] Clements (1916).
[55] Worster (1996), Chapter 10.
[56] Whittaker (1953) for instance, listed thirty-six different variations of the climax concept.

As formulated by Clements, the climax community is the end developmental state for a community of species (a meadow of grasses and shrubs for example). Over time, the constituent species were thought to change in a predictable manner. This sequence of change was known as succession and was composed of several seral stages. According to Clements, there were six laws of succession: (1) a succession resulted from the appearance of a new habitat or a striking change in an existing one. (2) Each stage of a succession acted on the habitat so as to produce conditions more or less unfavourable to itself but favourable to the invaders of the next stage. This second law meant, for example, that birch trees growing on Mount Robson in British Columbia, which were more able to recover after rock slides, would produce change that allowed conifers to take over. The slower growing conifers of the region could not survive because the rockslides occurred frequently enough that the conifers did not have time to develop. As the succession continued, the birch grew larger until they began to protect the conifers from rock damage. The birch, in effect, allowed the conifers to grow and out-compete the more light-requiring birches for sunlight.[57] (3) Initial formations were open: ultimate formations were closed. This meant that early successional stages changed, but later ones remained more and more constant until the final stage, which did not change unless a significant outside disturbance occurred. (4) The universal tendency of vegetation was towards stabilization. (5) The ultimate stage of a succession was determined by the dominant vegetation of the region. (6) The end of a succession was largely brought about by the progressive increase of competition, which made the entrance of invaders more and more difficult.[58]

---

[57] Plant succession in the Mount Robson region, British Columbia (1916).
[58] Bews (1916), 131.

Many ecologists used the concept of succession and climax to explain what they saw as the developmental aspects of nature. In early work, the imagery of competing forces, contests, and battles within an over-riding developmental frame (that is, the species that won were the strongest ones and therefore were supposed to win) was often used. A 1913 review of Cooper's work on island mosses of Lake Superior claimed that some species gained a foothold and advanced in three distinct lines of succession. Other species lost their dominance and were eliminated by competition. In one case, only one species was able to supersede the Cladonias. This species gradually spread itself out over the Cladonias "cutting off light and air and bringing about their [Cladonias'] death."[59] Species invaded. There were contests and struggles between species for existence. The reviewer concluded that the mosses were the weakest of all the species in the competition and could not hold their own against other types of larger more dominant vegetation. Justification for this structure was found in the notion of the climax; that is, nature was developmental because the species that emerged from the struggle as the dominant ones remained stable as climax species.

It is interesting that, as noted above, ecology during the first two decades of the twentieth century was attempting to establish itself as a science through an emphasis on quantitative data. The legitimate position for a subject of knowledge to speak in such a structure would at first seem to be a more distant and dispassionate position. However, at the same time as ecologists were calling for a more rigorous and scientific approach they were using the metaphorical language of battles with victors and vanquished. This kind of language continued into the 1960s.[60] In 1913, when Cooper's review was written, ecologists were distinguishing themselves from natural historians by using an over-arching explanatory framework. That this was not exactly an objective one mattered less than its very presence. Fieldworkers like Macoun and other natural historians of the time made little or no use of such frameworks. The climax model is an early example of the developing structure of ecological knowledge, and the most dramatic departure from the way in which natural historians approached their work was that the framework was comprehensive and offered explanations for change. Natural historians could not easily account for change. Furthermore, they were much less interested in the context of specimens. The locations in which and conditions under which a specimen was found might occupy a corner of the display tag on the case in the museum but little more. For ecologists, on the other hand, context was becoming more and more important.

---

[59] Ecological succession of mosses, Isle Royale, Lake Superior (1913), 202.

Part of this context was the classification of nature into zones. The concept of climax was useful in this process. Once the sequence of seral stages and the final climax community were known, areas could be identified as either in the process of change or in a stable climax. Knowing whether a species was a permanent fixture or not led ecologists to develop various organizing principles that placed species into a larger matrix of significance. So, for example, if a species could be identified as a temporary stage on the way to the climax, wherever its presence occurred, ecologists could automatically mark that area as transitional. A review of Cooper's 1916 book noted the use of the climax as an organizing principle for the vegetation of the Mount Robson.[61] Using the climax model, Cooper had classified the birch trees as transitional and commented that "the complete dominance of the climax conifers is now merely a matter of time." The climax model proved a powerful explanatory tool that continued to frame the studies, discussions, and debates in ecology for decades to come.[62]

---

[60] Martin (1959), for example, wrote about "[t]he conquest of the forest by hemlock" in Algonquin Park, Ontario, 216.

[61] Plant succession in the Mount Robson region, British Columbia (1916), 196.

Throughout most of the twentieth century, the use of climax and succession models was commonplace in ecology. All these models relied on and strengthened notions of progress, development, and stability. Statements, such as the "[c]limax prairie is a closed community,"[63] were used to show that once the climax state was reached, no further change was possible. This closure was, in fact, what many ecologists thought was the purpose of nature: develop to a point of stability where change no longer occurred.[64] The idea of succession was thought to be the general trend in nature. In 1940, Shantz claimed that all "plant cover passes through well-recognized stages of development or plant succession."[65] By the 1970s the dominance of the climax model was challenged, but for many years the concept of succession was a common framework for ecological studies. One year after Shantz, Brown went so far as to note that there was a correct and incorrect way to study nature: nature, he said, could not be understood unless we studied how it changed through time, by which he meant succession toward climax.[66]

While studying the Canadian prairie in the early 1950s, Coupland noted that in many cases the vegetation developed over time into a more stable community. The preceding stages were often vastly different and yet almost all led to a similar stable state. In the absence of outside influences (such as human disturbance), seral stages slowly changed into a stable one. It was possible for seral progression to be interrupted and the march toward climax might be derailed, but eventually the progression would reassert itself. The idea of outside disturbance was crucial in studies of agriculture, forests after fires, and insect outbreaks.[67] Disturbance was, by definition, a disruption. The ecological system would continue to progress unless interrupted. This definition framed disturbances as abnormal and, if not destructive, they at least presented a setback to natural systems. It was only when disturbance occurred that a climax community changed.[68] In sum, all paths led to the stable climax state unless something altered this progression: "[e]ach time the forest is destroyed, as by fire or logging, plant succession leading toward the same climax association is initiated once more because the fundamental characters of the habitat type are not permanently affected by disturbance."[69] In animal ecology, a similar emphasis can be seen. Charles Elton, in his seminal 1927 book on animal ecology, placed greater weight on those factors that regulated animal population numbers. For Elton, animal populations would grow out of control if there were not some kind of regulatory mechanisms (usually classed as density-dependent or -independent variables) keeping them in a stable balance.[70] The positive emphasis on stability here is evident in the language used. Population ecologists, like Elton, believed that anything that threw a community off track was an interruption. There was a purity of symmetry in the normal process of development that was lacking in the interrupted series. Stability became more highly valued and aesthetically pleasing than change, which was often described as violent, disruptive, combative, abrupt, an aberration, insidious, unnatural, damaging, or arresting.

---

[62] For example, Keever (1950).
[63] Weaver and Fitzpatrick (1934), 289.
[64] For other examples of succession and climax see Bard (1952), Beckwith (1954), Chapin III et al. (1994), Farrell (1991), Frelich and Reich (1995), Hayward (1948), Martin (1959), Mason (1947), McClure (1943), and Mohr (1943).
[65] Shantz (1940), 312.
[66] Brown (1941), 63.

---

[67] See Martin (1959).
[68] (Coupland 1950).
[69] Daubenmire (1952), 303. See also Menhinick (1967) for another example of the emphasis on stability.
[70] See Elton (1924), Elton and Nicholson (1942) and also the review of Elton's book by A. G. T. (1928).

As an object within and bound to ecological discursive practice, the climax model was produced within a broader context. Foucault asks if it is possible to lay down the rules to which the appearance of objects in discourse were subject. The first step in this process, he proposes, is to map the surfaces of their emergence: How were the models of succession and climax made possible? One aspect of the surface on which the model of climax emerged was the more general structure of science. This surface privileged certain relations between ecological objects. Science was progressive; its objective was truth. Truth was static; that is, the truer something was the less likely it was to be overturned. By refining its methods, techniques, instruments, and theories, ecology grew closer and closer to the truth about nature. In this way, stability came to be equated with truth: the more stable, the truer. Objects in ecology were related to each other through the rules of stability and development that modern scientific discourse had already established. The climax model was a powerful element in ecological discourse because it clearly drew from and supported notions of truth and stability that were themselves deeply embedded in scientific and modernist discourse in general. The model demonstrated a beautiful structural symmetry that linked development and stability with the truth about nature: "[c]ommunity stability increases," Pimentel commented in 1966, "as the community approaches the climax stage of succession."[71] This linkage meant the previous seral stages were immature versions of the final product.

The idea that nature was progressing toward a stable end point became, in the 1960s and 1970s, a central component of OR's conceptualization of low impact. Natural areas that continued to change could not have reached their final end point; thus, their development must be being blocked by something. Combined with this idea was the way disturbance was conceptualized as a negative force. Added together, the interplay between development, progress, and disturbance led to the conclusion that human actions were the main force preventing wild areas from developing as they should. This emphasis is not necessarily wrong; in fact, there are many examples of human action destroying a natural system. The point is to realise that the surface from which OR's conclusions, about the influence humans have on nature, arose had already divided stability from change in such a way that stability was privileged. This division formed a condition, not an object, for thinking about humans in wilderness. As Foucault has noted, division is often accompanied by rejection, and that which is rejected is by definition false or untrue. Science divides that which is true from that which is false and at the same time accepts that which is true and rejects that which is false. In the case of disturbance and stability in ecological science, the former was rejected in favour of the latter. As a result, when OR researchers turned to ecological science for insights into the structure and function of natural systems, they unknowingly subscribed to a previous division and rejection that had already determined that stability was the true state of maturely developed natural systems while disturbances represented a degraded or even false state of nature. While the validity of this division and rejection is open to criticisms at some level, we must bear in mind that in many cases natural systems that have been altered significantly by human over-use are actually less viable in many ways than those that are still functioning without the extreme impacts seen in many wilderness areas in Canada today.

---

[71] Pimentel (1966), 29.

Because knowledge in ecological discourse was seen to be progressive, cases where evidence challenged the climax concept could be re-interpreted as more support for the developmental character of ecology: contrary evidence was often seen as stemming from a paucity of knowledge brought about by insufficient sophistication in methods, instruments, or techniques.[72] This re-interpretation represents a twist on the discursive practice of division and rejection. Instead of accepting only the true and rejecting all the false, ecology eliminated the false by absorbing it into the true. As the discipline of ecology developed, the claim was that this contrary evidence would be assimilated into the succession/climax model. Thus, even the conflicts were seen as evidence for the progress that ecology was making.

However, until ecology could agree on a comprehensive model of succession and climax, the disagreements and conflicting evidence needed to be addressed. One example of the disagreements that arose occurred between Clements and Tansley. According to Clements, a formation was "the climax community of a natural area in which the essential climatic relations are similar or identical. It is delimited chiefly by development."[73] Tansley criticized this understanding of formation and climax because it neglected the great number of instances where deviations occurred: for example, "the far-reaching 'telescoping' of phases, the appearance of plants or populations out of their 'proper' order and so on."[74] Even with this criticism, however, Tansley still supported climax theory.

---

[72] An example of this re-interpretation in relation to succession can be found in Laessle (1958).

[73] Quoted in A. G. T. (1916), 199.

[74] A. G. T. (1916), 201. See further, Tansley's later work A. G. T. (1922) where he stated his disagreement "with Clements in regarding the climatic climax as the only climaxes to be recognised as such," 248. According to McIntosh (1975), Gleason also disagreed with Clements. In 1910, he stated that "'it is impossible to state whether there is one definitive climax association in each province; it seems probably that there are several such associations each characteristic of a limited portion.' Thus, he [Gleason] clearly came out against the monoclimax concept proposed by Clements and endorsed a much less rigid view. [In 1927, he] asserted, 'succession is an extraordinarily mobile phenomenon, whose processes are not to be

The problem, he claimed, was not that the theory was inherently flawed. The problem lay with "successional phenomena which are often exceedingly hard to establish, especially in regions where the vegetation has been modified artificially to a large and unknown extent."[75] Presumably, following this line of reasoning, once we learn more about successional phenomena, we will be able to identify formations and climax states.

Brenchley and Adam went even further with Tansley's argument that it was difficult to identify climax states. In their 1915 study of the vegetation in Broadbalk wilderness, they observed that the community was in a state of flux (and therefore not a climax). Because of their lack of knowledge about the seral stages, they concluded, it was not simply difficult but "impossible to forecast the direction that the change will follow."[76] Even by 1951, Mentzer noted that little was "known about the relationship between total yield of grassland and its stage of development toward the climax."[77] Nature was mysterious and unpredictable, and these traits were attributed to lack of knowledge. The most common view of nature for at least the first seventy to eighty years of the 1900s was as a balanced and stable system; consequently, because ecologists believed their models were correct, their inability to predict changes in seres had, in their minds, to be due to ignorance—in effect, they were saying, they had not looked hard enough, long enough, or with the right instruments.

So prominent was the model of succession to climax that even when evidence began to accumulate against it, ecologists continued to have faith in it:

---

stated as fixed laws, but only as general principles of exceedingly broad nature, and whose results need not, and frequently do not, ensue in any definitely predictable way,'" 255.

[75] A. G. T. (1916), 203.

[76] Brenchley and Adam (1915), 200.

[77] Mentzer (1951), 261.

> [a] community is considered *seral* if interrupted age-gradients in species populations show that the sociologic status of at least some of the species is temporary, but *climax* if it appears to be self-regenerating and there is no concrete evidence that it is followed by a different subsequent community. Actually, no vegetation is absolutely permanent, but a reasonable and useful line of demarkation can be drawn between succession that is demonstrable and predictable, and that which is brought about by unforseeable events and therefore unpredictable.[78]

As Daubenmire indicated, there can be no data indicating anything following a climax community; however, no vegetation is permanent and therefore something must follow all climax states. In order to address this conflict, he distinguished between predictable and unpredictable seral stages. This distinction, unfortunately, did not address the concern he raised that climaxes are supposed to be permanent but cannot be. Nonetheless, Daubenmire and others continued to search for successional stages and climaxes. In 1954, two years after Daubenmire, Beckwith commented that "[p]lant and animal succession does not always proceed along the lines which have been indicated above. Numerous influences sometimes enter in and alter the course and rate of succession."[79] Furthermore, the influence of variations in weather or changes in drainage conditions on the development of plant cover could not be determined due to lack of data, "but the importance of these factors in affecting the course of succession is evident." This lack of data made it "impossible to determine the end of this stage of succession."[80] Although there seemed to be serious problems with the model of succession and climax in this study, Beckwith still concluded that "the vegetation passes through successional stages in the following order: Annual-biennial, perennial grass, mixed herbaceous perennial, shrub, and intolerant tree."[81]

---

[78] Daubenmire (1952), 302.
[79] Beckwith (1954), 372.
[80] Beckwith (1954), 371.

Although succession and climax had their detractors who eventually prevailed, by 1969 Eugene Odum could still write that succession had three basic characteristics: "1) it is orderly, reasonably directed, and predictable; 2) it is controlled by the biological community, even though the basic constraints upon it are set by the physical environment; and 3) it culminates in a community in which the 'maximum biomass ... and symbiotic function between organisms are maintained per unit of energy flow.'"[82] The reasons, I suggest, that the ideas of climax and succession remained popular even as more and more evidence accumulated against them are many. Certainly a significant portion of those reasons lies in the rules of formation discussed above. As a science, ecology stressed development, progress, prediction, and control. These figures comprised the surface upon which the models of climax and succession emerged and were arranged. The climax concept played into this structure: once the stages were outlined, future changes could be predicted and manipulated (controlled) to a variety of ends. Furthermore, knowing the end point allowed ecologists to fit communities into an overall developmental progression. The closer the community was to climax, the more stable it was. Stability became the pinnacle of development.[83] This led to Tansley's conceptualisation of unstable communities as immature while ones that approached or attained climax were more mature.[84] Finally, as Worster noted, climax theory was a progressive theory and the notion of progress has a long history in modernity. Climax theory, in its suggestion and promise that man can learn to create the perfect system, fit well with notions that human rationality can overcome problems, come to understand the universe/world, and develop solutions without relying on God.[85]

---

[81] Beckwith (1954), 375.
[82] Quoted in Ehrlich and Roughgarden (1987), 542.
[83] See Rene Dubos, quoted in McDonald (1970), who claimed that "[a]ll ecological systems, whether man made or natural, must in the long run achieve a state of equilibrium and be both self-regulating with regard to both energy and materials," 10.
[84] Tansley (1939).

Climax states were stable was because they were seen as balanced and harmonious: "[n]ature has brought together associations of plants which live together in general harmony, often aiding each other in various ways, and always contriving to preserve a cover over the soil throughout the year."[86] It was not just associations that were stable: "[c]ommunities are made up of groups of species which become integrated to react more or less as a supra-organism.... This means that gradually species have evolved together to function as one balanced unit."[87] The idea of balance was essential to the climax model and has been variously called the balance of nature, the ecological balance, equilibrium state, steady-state, and homeostasis.

Eugene Odum added another dimension to the concept of balanced nature. He suggested that "[s]ince ecological structure is readily shown in a diagram but difficult to depict in the language of pure mathematics, the electrical analogue circuit diagram or working model has the advantage of simplicity in relating structure and function."[88] Odum's circuit and, even more so, his universal thermodynamic models explain the structure of all (eco)systems (Figure 2.1).

---

[85] Worster (1994).
[86] (Thornthwaite 1940), 345.
[87] (Pimentel 1966), 25.
[88] (Odum 1964), 16.

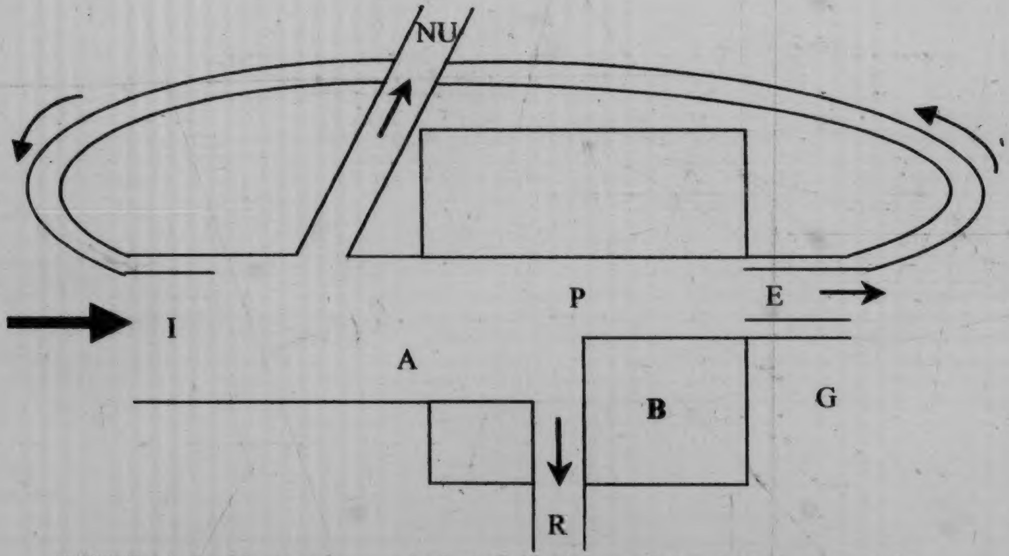

FIGURE 2.1. Partitioning of energy in the individual or population. I, input or ingested energy; NU, not used; A, assimilated energy; P, production; R, respiration; B, biomass; G, growth; E, excreted energy.[89]

Every (eco)system, he argued, should be thought of in terms of energy input, circulation, respiration, use, and waste. The diagrams based on these models were to be universal in various scopes (temporal, dimensional, and geographical). Odum measured the incoming amount of energy and the energy required in order to create a balance sheet for any community. One of the earliest examples of energy models was Howard Odum's 1957 study on Silver Springs. He concluded that this community was in a steady-state because it used the same amount of energy as it had coming in. In other words, income balanced expenses.[90] Studies such as Odum's energy flow chart for Silver Springs became popular in the mid-1950s. Most often, these studies used energetics to show whether a community was balanced or not. If the amount of energy needed was greater than the supply, the system was not self-sustaining.[91]

---

[89] (Odum 1993), 97.

[90] H. Odum (1957). See also Eugene Odum's influential textbook series that began in 1953 and continued into the 1990s (Odum 1953, 1959, 1971, 1983, and 1993).

Probably the most influential early energy study was done in 1942 by Raymond Lindeman.[92] In this work the trophic-dynamic concept received its first clear delineation. Nearly all subsequent studies on energetics relied to a greater or lesser extent on Lindeman's model of trophic-dynamics.[93] Simply put, it divided all communities not into associations, populations, or formations as most everyone else did, but into energy levels. At the most basic level, the bottom one in Lindeman's model, was the producers, the group of organisms that produced energy directly from the system's input. Producers were most often the plant life that converted sunlight into energy through photosynthesis. Other levels consisted of organisms that satisfied their energy requirements by consuming other organisms; hence, they were called consumers.

What Lindeman, and others since, had been most interested in was the question of efficiency: how much energy is lost in the transfer from one trophic level to another? It was known to ecologists that no energy transfer was perfect, and thus, some must be lost along the way: but how much and where? Ecologists needed to know what the efficiency rates were in order to construct accurate energy balance sheets. Without knowing how much was lost, ecologists could not account for all the energy expenditures and, thus, no system would be balanced, not even climax ones. Since everyone knew that climax systems were balanced (according to the theory at the time), some explanation for the energy loss was needed. Odum and others began their investigations by trying to determine trophic efficiencies in order to find support for the idea of balanced systems. Lindeman's ideas on trophic-dynamics, if proven correct, would also lend support to the progressive formulation of succession: trophic transfer efficiency rates were postulated to increase through the various seres leading to the climax state.

---

[91] See also Golley (1960), Hadley and Bliss (1964), Menhinick (1967), Teal (1957), and Wiegert (1964) for other studies of community energetics.
[92] Lindeman (1942). See also Cook (1977) for a discussion of the importance of Lindeman's work.
[93] For a more recent example, see Breiturg et al. (1997).

Studies of individual organisms' metabolisms began appearing at the same time as studies of community metabolism. Porter and Gates, for example, concluded that "[a]ny organism must be in thermodynamic equilibrium when averaged over a reasonable length of time in order to survive."[94] Note the similarity between their comment and Howard Odum's that

> [i]n any community the influx of energy must be entirely accounted for in the passage through the community and in the outflow if an understanding of the community metabolism is to be obtained.... In a steady state community, this objective may in part be realized [by constructing a balance sheet of the various energy estimates].[95]

For these authors, balanced systems, whether an individual animal or community, were characterized by steady-state energy flows. Balance or equilibrium was required for survival; therefore, all systems (groups and individuals) that were balanced were also in a climax state because they persisted, whereas imbalanced ones collapsed.

Finally, for Eugene and Howard Odum, "[t]he production [energy gain] on the reef seems to about balance the respiration [energy loss] on the reef.... It is concluded that the reef community is ... a true ecological climax or open steady-state system."[96] That they call a balanced community a *true* ecological system demonstrates the importance of balance and stability. According to the climax theory, anything in nature that was not balanced could not be a *true* climax. The implication that unbalanced nature was untrue or false seemed not to concern ecologists until well into the 1960s. Coincidentally, it was also the 1960s that saw OR begin to study impacts more intensively. The idea that nature out of balance was not the true form of nature reinforced the conceptualization of humans as a disturbance to nature. When humans enter into natural systems, they throw off the balance of nature. The result of this thinking can be clearly seen in the studies OR undertook in an attempt to reveal the impacts people were having and to raise awareness that these impacts had to be curbed or else all naturally functioning wilderness areas would disappear.

---

[94] Porter and Gates (1969), 242.
[95] H. Odum (1957), 106.
[96] Odum and Odum (1955), 319. See also Hadley and Bliss (1964).

Mechanisms of Universalization and Standardization

> *Humans are now more powerful than natural selection.*
> *~ Peter Outerbridge*

One of the most strikingly common occurrences in ecology in the first sixty years of the twentieth century is the debate over terms and definitions.[97] Ecologists argued over the meaning of certain terms and regularly lamented the lack of consistency their field displayed. Ecology could not, it was felt, mature as a science as long as its practitioners were speaking numerous languages. As early as 1914, Rubel give voice to this concern when he commented that "if every nation continues to use the terms of its own language, then in future the expression of the larger relationships—the chief thing for universal science—will be lost."[98] Given that the most common model of nature was seral stages toward a stable climax, it is not surprising that the debate over consistency focussed mainly on this model. Before ecologists could determine the developmental stages an area would pass through on its way toward the climax state, they had to agree on a classification system for the units that would undergo these seral changes. It would be mistaken, however, to think that after Clements' 1916 work on plant succession the terms were set and the sequence accepted. What happened instead was that Clements provided a detailed system for succession, which then formed the basic configuration for subsequent discussions. After Clements, the number of competing ideas and terms surrounding the succession/climax debate did not decrease; they increased.

---

[97] Selective examples include *Prinzipienfragen des vegetationsforschung. Ein beitrag zur begriffsklarung und methodik der biocoenologie* (1919), Allan (1926), Egler (1942), Margalef (1963), Solomon (1949), Tansley (1939), and Taylor (1935).
[98] Rubel (1914b), 232.

In order for ecological terms to be useful, they must meet a number of criteria. First, they must be clearly and consistently defined. As Nichols explained, "it is necessary that the various concepts upon which the classification [of plant communities] is to be based should be definitely formulated; and for convenience in expressing concepts of any description it is imperative that a rather definite terminology should be adopted."[99] Terms also need to be arranged in set relations with other terms. They need, in other words, to be put in their place. Again, Nichols: "[c]lassification, generally speaking, may be described as the arrangement into groups of objects or phenomena which are related to one another by the possession of certain characters in common."[100] These relations need to be set and agreed upon as much as possible. Thirdly, terms also should be of continuing value: current terms should be able to accommodate the results of future experiments or discoveries. For example, if a meadow underwent a certain progression of stages before finally becoming a mature forest, then we should see this pattern played out in the future as new but similar meadows are studied. Finally, and underlying all other criteria, each and every one of these terms must refer to real things.

---

[99] Nichols (1923a), 12. See Pearsall (1918) for a similar discussion but related to a classification system for aquatic communities.

[100] Nichols (1923b), 154.

They must be factual and describe something in the world. Unfortunately, the terms ecologists were using had a difficult time meeting these criteria.

To be clear, that these terms did not measure up was not the fault of the ecologists, as if they were biased against certain terms or held a grudge against other ecologists. The problem was not the improper application of method either. Nor, finally, was it an issue of intelligence or lack of knowledge. These troubling terms arose, I think, because the structure of modern science placed certain demands on discourse that could not be lived up to. When asked to do too much, concepts, such as succession, rebelled. They refused to stand still and be counted as autonomous, unchanging bits of reality. Because of this rebelliousness, it was difficult to pin them down to specific meanings and definitions, never mind universalize them. However, this difficulty did not stop ecologists from trying, and in the process they engaged in many fruitful disagreements.

Some of the early attempts at designing a universal schema for classifying nature were developed without any indication of these problems. In his 1918 study of moors and their relations to grasslands, Adamson concluded that "the plant associations of the district fall naturally into three main groups."[101] For Adamson, these groupings were natural and nearly self-evident. Dachnowski seemed to agree. In 1914, while travelling across the United States observing the vegetation, he wondered why ecology had not been able to come to "some definite agreement as to the nature of vegetation units or as to the method of classifying them." His puzzlement came from the belief that "[e]ach association is large enough to form in some measure a complete entity with a relative permanence of relations among the component parts throughout the area."[102] If this belief was so, it should have been a relatively simple task to outline these entities and their relations to one another. Unfortunately, outlining the boundaries between associations turned out to be much more difficult than Dachnowski suspected. The problem was not a paucity of information about associations. On the contrary, there was an overabundance of information. Individual ecologists were quite clear as to the meaning of certain terms; however, comparing meanings between authors shows not only multiple and shifting meanings, but also contradictory and incompatible ones.[103]

---

[101] Adamson (1918), 109. Not every ecologist agreed it was this easy to classify nature. In 1913 Moss chastised Henslow for having "an underlying and *naive* assumption that it is quite an easy matter to refer plants to these classes [xerophytes, hygrophytes, halophytes, and mesophytes]" (Moss 1913, 293).

One of these classification systems appeared in Drude's 1913 textbook on plant ecology. Drude adopted the following system of classification (from smallest to largest): elementary associations, associations, formations, formation-groups, and vegetation-types.[104] An association, he claimed, has a definite floristic character (the combination of species present, including dominant or diagnostic ones), without which that association would not be the same association. Formations, on the other hand, are characterized by a definite physiognomic (structural) type. The example Drude gave of a formation was that of the eastern North American deciduous forests. This formation was characterized by deciduous trees of many different species in all combinations. The distinction between an association and a formation could not be clearer it seemed: the first was based on floristic traits, the other on physiognomic ones. For Dachnowski, too, associations were clearly defined. For example, forests were associations because they were readily distinguishable, whereas plants on dunes were not. "Such populations of plants, retrogressive from forests and surviving partial burial by dunes, are not, of course, a distinct social entity, and hence cannot be given the rank of a society or of an association."[105] The entities Dachnowski accorded the status of association had an obvious existence as discrete units, which is similar to the way Drude saw these entities.

---

[102] Dachnowski (1914), 240. See also Rubel (1914a) who claimed that "[i]n a continental climate the vegetation limits are generally well marked and easy to determine," 41.
[103] Shelford (1935) offers some examples of this multiplicity of meanings when he compared animal ecologists to plant ecologists.
[104] In a review by A. G. T. (1914).

Deciding what kind of entity formations, associations, and communities were marked much of the debate in the earlier decades of the twentieth century.[106] Clements clearly fell on one side of the debate: "the unit or climax formation is an organic entity … [that] arises, grows, matures, and dies.… Furthermore, each climax formation is able to reproduce itself, repeating with essential fidelity the stages of its development."[107] This definition made the formation concept seem unambiguous and obvious, so much so that for Clements the debate was over. As Colinvaux has noted,

> [f]or Clements (1916), the intellectual prize of a general classification of plant communities seemed to be won. There were a few great climax plant communities in the world, the formations … all other communities within the formation boundaries were but seral stages of various successions which should lead to the areal climax or formation. And this satisfying classification apparently led to a new truth. The formation had many of the characteristics of an organism; the whole was more than the sum of the parts.[108]

As Foucault has noted in *Archaeology of Knowledge*, all statements have a field of emergence, have the authority to differentiate, can be assigned the function of a subject who is not the same as the emitter of the sentence, and are part of a larger enunciative field. We can understand this particular enunciation by Clements as having emerged within/from the more general field of scientific knowledge and modernity. The subject position from which a statement with the power to differentiate between the correct and incorrect view of nature could be articulated would have been a privileged one that was aligned with other powerful discursive forces.[109] Clements, as a respected member of the establishment, as a logical and detailed scientist, and as an impassionate (some would say 'dry') writer who built arguments up from detailed observations and reams of data, occupied such a subject position: the expert scientist. This position of privilege was a modern one. Modernity's larger surface created the very possibility for such subject positions, prior to anyone actually occupying them. This surface privileged inductive rationality, autonomy, and individualism, along with those disciplines that propounded developmental models of maturation, such as psychology and education. The position Clements occupied was built upon this pre-existing surface.

---

[105] Dachnowski (1914), 242.

[106] See also the anonymous review *Vorschlag zur nomenklatur der soziologischen pflanzengeographie* (1919) for a discussion related to developing the criteria for classifying plant associations. For an overview of the main points in this debate, see the textbook by Begon, Harper, and Townsend (1990) in which the authors noted that "[m]any ecologists have been preoccupied with the idea of community boundaries. Indeed, there had been much debate and concern over whether community ecology can legitimately be studied at all if communities do not exist as definable units," 628.

[107] Clements (1916), 3.

[108] Colinvaux (1973), 79 and 86. The idea that the whole is more than the sum of its parts forms the centrepiece for much of the debate during this time. Golley's history of ecological thought indicates the significance of this idea by using it for its title: *A History of the Ecosystem Concept in Ecology: More than the Sum of the Parts* (Golley, 1993).

Foucault also noted that statements have a materiality that can be repeated; however, the enunciation of a statement is always a unique event. In other words, we can re-print a book, but each time it is read, it is enunciated anew. Part of the materiality of Clements' statements on formations was the proliferation of the books he wrote and the size of their print runs. His 1916 book on plant succession was one of the most important ecological works for nearly the first half of the 1900s. It is still found referenced in nearly all textbooks and the ideas in it are circulating widely even today. The materiality of statements about nature also manifest in nature itself. Studies were designed to determine whether or not nature followed the Clementsian model. Scholars began to investigate other dimensions of nature under the assumption that nature was Clementsian in its development and arrangement. Other scholars were sceptical of Clements' model and conducted studies to disprove his model of succession. Studies for or against Clements' model had many material effects. Areas were set aside as particularly good examples of climaxes. In the 1950s and 1960s, terrestrial ecology studies required the irradiating of forests to test the radiation's impact on succession. Crops were planted and soil treated according to the predictions of the climax model, and chemical applications were undertaken based on the understanding that formations and associations were mechanistic or organic entities; that is, formations and associations were seen either as machines with independent parts that could be removed or as organic wholes that could not sustain the removal of any components.

---

[109] There is an important distinction between subjects and subject positions. Subject positions are locations, not individuals. Privileged subject positions are locations in any discursive field that increase one's power or influence. Anyone who occupies a subject position (privileged or otherwise) is a subject who speaks from a specific and identifiable location in the discourse. In Clements's situation, he is the subject that occupies the privileged subject position of expert scientist.

For Clements, in 1916, climax communities were clearly organisms. However, only one year later Gleason was arguing quite heatedly against the organismic conceptualization of formations. Gleason claimed his view was "in sharp contrast with the view of Clements that the unit of vegetation is an organism, which exhibits a series of functions distinct from those of the individual and within which the individual plants play a part as subsidiary to the whole."[110] Gleason endorsed the understanding that an association was an aggregate of individuals. There was no super-organism. In fact, he asked whether, because of all the contradictory evidence, we should say that "an association is not an organism, scarcely even a vegetational unit, but merely a

---

[110] Gleason (1917), 464.

coincidence?"[111] And then, in 1947, Cain put forth another view. He criticized ecologists for their tacit assumption that the association is floristically characterized and has some type of objective reality. There were many known instances of floristic assemblages of numerous stands that were typical of neither any known association nor transitional between any two. "I cannot see," he concluded, "that the association, as usually understood in either the large or the small sense, has objective reality. That which passes best as an association is a series of stands in a local area where there is an overlap of the areas of a series of species and each species is equally available on the various sites."[112] Gleason and Clements at least agreed on one thing: plant associations existed. Their existence was a certainty, Gleason noted, because "we can walk over them, we can measure their extent, we can describe their structure in terms of their component species, we can correlate them with their environment, we can frequently discover their past history and make inferences about their future."[113] Cain, however, saw them as having no objective existence at all.[114]

In the quest for universalized and standardized terms and models, the viewpoints of Gleason, Clements, and Cain could not all exist and be correct. One had to choose sides: either associations were organisms, or they were aggregates of individuals, or, finally, they did not exist. If terms had to be consistent, related clearly to other terms (set in a web significance with overt relations), have predictive value, and refer to real things, then the debate over 'association' indicates success in none of these requirements. The term was not consistently defined. Even within a single author there were discrepancies: Clements, for instance, described formations one way in 1905 and another in 1916.[115] The terms 'association' and 'formation' existed in complicated and inconsistent relations to each other. The closest that ecology came to employing these terms successfully came in regard to the third criterion: predictive value. Associations and formations were both part of the general sequences of developmental stages leading up to a climax; however, no one could agree on what constituted a climax state. At one point thirty-six different climax states were in circulation in the literature.[116] In such a convoluted system, the predictive value of the term 'association' was limited. Furthermore, if, as Cain claimed, associations have no objective reality, then they cannot very well be predictive of future vegetation states.

---

[111] Gleason (1917 and 1926). See McIntosh (1975) for a detailed discussion of Gleason's contributions to ecology and his status within that community.

[112] Cain (1947), 195.

[113] Gleason (1926), 7.

[114] The presence of different and competing views on the status of associations does not indicate a linear development in the ideas surrounding the nature of ecological units. For example, Gleason wrote formally about the individualistic concept as early as 1917 (although its beginnings can be seen in his 1910 publication), but it was Clements' viewpoint that predominated the first half of the twentieth century. Gleason, in fact, commented in 1926 that "for ten years or thereabout, I was an ecological outlaw, sometimes referred to as a 'good man gone wrong'" (McIntosh, 1975, 259). Then, in 1958, Friederichs revived the individualistic view and even went as far as to argue that "[i]t has long been demonstrated … that the ecological units are something else than organisms" (Friederichs, 1958, 155). Although Gleason had put forth this idea in 1917, it was not, as Friederichs claimed, demonstrated conclusively. For examples of counter-points, see Emerson (1939) and Taylor (1935) for two persuasive arguments in favour of the super-organism conceptualization of vegetation.

Clements postulated, in addition to suggesting that plant formations were organismic, that formations could be traced and analysed only by physiognomic, floristic, and habitat means.[117] This postulation was opposed to Drude's 1913 conceptualization that formations were defined only by their physiognomic characteristics, not floristic and habitat factors. Twenty-five years later the problem still had not been resolved. Brown, in his 1941 study of the vegetation of Roan Mountain in the Southern Appalachians, equated community with association and noted that "[i]n this study plant communities are characterized by a distinct physiognomy and a definite floristic composition."[118] In 1913, formations were distinguished by physiognomy not floristics. In 1916, formations were the same as communities and could be distinguished through both physiognomy and floristics. By 1941 communities could still be characterized by both physiognomy and floristic measures, but they were no longer formations; they were now associations, which was contradictory to Drude's system in which only floristics determined an association.

---

[115] See the review by A. G. T. (1916) of Clements' 1916 book in which Tansley notes, with some disapproval, this discrepancy.

[116] See Whittaker (1953).

[117] A. G. T. (1916), 199.

The terms and concepts related to succession and climax were not the only site for debates over terminology. In the 1950s, population ecology paused to take stock of where the field was and to try more clearly to define its terms and concepts. The field was still quite fractious, with ecologists taking a variety of positions: "[n]ot only were they uneasy about the theoretical part of their science, they were not at all agreed as to the status of the facts themselves.... In trying to remove ambiguities, they argued incessantly over what words meant and whether a given idea signified something real or illusory."[119] These debates concerned more than just the meaning of concepts; they contested the very existence of certain central concepts. For example, "W. R. Thompson ... argued that the population existed only in the mind and head and had no objective reality."[120] It was difficult, to say the least, to reconcile this position with those that claimed an independent existence for populations. It should be noted that those ecologists arguing against the population concept were not suggesting a radical type of contingency; they were arguing in favour of their own schematic, one that was different but just as factual.

---

[118] Brown (1941), 67.
[119] Kingsland (1985), 3.
[120] Kingsland (1985), 173.

This debate over basic terms in ecology stemmed from the perceived need to generate standardized models that would be applicable to nature wherever it was studied. That the concepts of formation and association came to be the most debated and contested terms is not surprising. They formed the fundamental units used by ecologists to study of nature. According to Macnab, associations and formations constituted "the fundamental structure, organization, and interrelationship of … communities."[121] Daudenmire averred that "[t]he association is considered the basic unit of vegetation classification."[122] Once this structure was known and a classification system developed, communities, associations, or formations in different areas could be compared.

Perhaps the best explanation of the requirements of classification was voiced by Nichols in 1923:

> [t]o fulfill scientific requirements a scheme of classification must be logical in its concepts, clear-cut in its definitions, and consistent in its methods; furthermore, it should be 'natural' to the extent that it is based on principles and relationships which exist in nature. To be of practical value such a scheme must possess sufficient elasticity to render it adaptable to a wide range of conditions and to varying points of view.[123]

Standardization, individuation, and normalization form cornerstones in the classification process. In addition, we can see the strictures placed on the meaning of terms in this passage: for any classification system to work, each term in it needed to be consistently defined, be arranged in set relations with other terms, and refer to real things. As Miller, noted, there is great danger in refusing to develop a classification system: "without

---

[121] Macnab (1958), 21.
[122] Daubenmire (1952), 300.
[123] Nichols (1923b), 154-155.

[classification] all knowledge would exist as a disorderly and shapeless mass, too huge for the memory to grasp and too heterogeneous for the understanding to employ."[124]

Miller's comment reveals one way that the drive to science, with its emphasis on standardization, encouraged research in particular directions. The fear was that without a scientific methodology the entire discipline would dissolve into a shapeless mass of opinion and idiosyncratic perspectives. The task for ecologists, then, was to give form to nature and arrange knowledge into discrete categories with logical connections. This, in turn, would prove ecology a science and ward off criticisms that it was only a pseudo-science. This fear proved particularly acute for ecologists, as theirs was a science of the field just as much as of the laboratory. Traditional models of science emphasized controlled experiments in which variables could be manipulated one at a time. In ecological studies of natural systems this level of control was difficult at best.

One of the best known ecologists of the first half of the twentieth century, Victor Shelford, expressed this concern in his 1934 article titled 'Faith in the Results of Controlled Laboratory Experiments as Applied in Nature.'[125] Shelford argued that ecology was a mature science because it was possible to make outdoor experiments scientific if certain procedures were followed, namely controlled observation, comparative study, and the adoption of standardized methodologies and terminology. He argued, in effect, that it was possible to take the essential points of a laboratory experiment and move them outdoors. The use of the word 'faith' in his title, however, calls attention to the precarious position ecology held for much of its history. Ecology occupied a unique position in the academic structure: it was not part of the humanities and yet did not fit neatly into the established sciences. For one thing, most sciences operated along a reductionist axis; ecology, on the other hand, was often holistic or synthesizing in its attempts to assemble complete systems. Ironically, the natural collectors of the previous century, from whom ecologists wished to distance themselves, were closer in at least one respect to the scientific standard than ecology. In a fashion similar to reductionist biology, natural collectors removed objects from their context and studied them in isolation. With the advent of ecology and its emphasis on context, the privileged position of the laboratory as the only place to conduct scientific experiments was challenged.

---

[124] Hugh Miller, quoted in Nichols (1923b), 154. See also Thornthwaite (1931) for another example of the centrality of classification in ecological studies.
[125] Shelford (1934).

Although ecology presented an interesting challenge to the established sciences, there were still many attempts to legitimate itself through an alliance with these sciences. For example, in 1957 a prominent ecologist, Howard Odum, argued that Silver Springs constituted "a giant constant temperature laboratory.... In this rare situation it is possible to compare whole communities in a ready made experimental design."[126] This argument reflects the drive to science that manifested itself periodically in ecological literature throughout the 1900s. Even in the textbooks of the 1970s-1990s there often appeared, in the introductory section, comments about the place and role of ecology in the sciences.[127] The arguments that were put forth speak to an underlying anxiety that perhaps ecology would not be taken seriously because it was not sufficiently standardized or did not follow the strictures of science close enough. The preoccupation with and emphasis on quantitative and objective data reflected this anxiety. Most ecological predictions were not as accurate as predictions in other sciences (for instance, no ecological study could match the success of the laws in physics or chemistry). This potentially damming realisation was interpreted as evidence for the immaturity of the discipline, not its inherent inability to achieve such predictions. The figures of progress and development framed this potential shortcoming as a strength: ecologists could and did argue that their discipline was advancing and that their failures were actually evidence of such progress.

---

[126] H. Odum (1957), 55.

[127] For example, Begon, Harper, and Townsend (1990), Colinvaux (1973), Ehrlich and Roughgarden (1987), and Ricklefs (1979).

A Disciplined View: Deep Ecological Vision

> *The great tragedy of science—the slaying of a beautiful hypothesis by an ugly fact. ~ T. H. Huxley, 'Biogenesis and Abiogenesis'*

Ecology came into its own as it became distinct from older understandings of nature and humanity's place in it. These older, more religious, understandings of nature delayed the adoption of Darwinian theory in Canada for a few decades compared to its adoption in the United States.[128] John Macoun, for instance, encouraged the practice of natural science in Canada for over thirty years after the scientific community in the USA and Britain had accepted more modern understandings of nature. Macoun began his work when nature was seen as mysterious, wonderful, and potentially dangerous.[129] Understandings of nature in the 1880s were limited in part by the belief that the human mind was not equipped to pierce the veils that shrouded nature's mysteries. Prior to the widespread adoption of a modern, secular, ecological view, knowledge about nature was organised by religious beliefs about the purpose and place of nature and people just as much as it was by scientific standards.

---

[128] See Waiser (1989) for a discussion of Macoun's influence vis-à-vis Darwin's theory of evolution.

[129] See McKillop (1979), especially Chapter Four 'The Veils of Isis,' for a discussion of the prevailing attitude toward nature, biology, science, and religion in Canada. Berger (1983) also provides a discussion on this topic.

As a more scientific view became common, the questions ecologists were asking changed. Ecologists began to analyse the structure, interconnections, and functions of nature. When using this approach, ecologists thought they were seeing into the depths of nature in a particular way. Natural historians who believed that God's hand was evident in the beauty of nature also believed they saw into the heart of nature, but there were some dramatic differences between what ecologists and natural historians saw. When ecologists studied nature using science, they entered into a set of power relations with the objects of ecological discourse. The orientation of the scientist to the object of study re-organised the asymmetry between nature and people. Nature was now seen as something decipherable. It may still be unknown and mysterious, but the solution to our ignorance was within our own hands. Religion and God were removed from the rational study of nature. Nature was composed of objects out there in the real world that functioned according to laws that themselves were rational and comprehensible. It became a matter of accumulating enough data of the right kind.

As ecology developed in sophistication, it began to refine its techniques for observation. In the early part of its history, ecology often contained counts of animals or plants. Descriptions of colour, texture, and patterns were common. In 1914, for example, Rowan studied the number, shape, size, and colours of eggs in a tern colony.[130] Through his study, Rowan hoped to ascertain what, if any, laws were governing the laying of eggs. This study marks an interesting cusp: although mainly descriptive of observed patterns, Rowan, nonetheless, was seeking universal laws. Laws of nature are not directly observable; they underlie the observable regularities. Over time, this emphasis on the underlying aspects of nature became more and more pronounced, and ecologists attempted to peer further into nature's secrets as they sorted through a mass of unclassified and undifferentiated information. As a result, ecologists quickly began to search beyond observations of what species were present in what numbers for answers to questions about how nature functioned. Part of understanding nature's functioning was to understand its structure. "No study of the vegetation is complete," Brown surmised in 1941, "that does not take into account its structural and developmental aspects."[131] There are different levels of structural analysis, of course, but the deeper-level structures were seen as underlying and uniting surface-level differences.

---

[130] Rowan (1914).

Christopher Norris has explained the power of structure by noting that "[o]ld polemics are quietly forgotten because the ground has meanwhile shifted to such an extent that erstwhile opponents find themselves now in a state of peaceful alliance."[132] This was, in fact, the hallmark of structural analysis in ecology: it could explain how nature appeared in a variety of settings. The disciplinary pressures to achieve ever higher levels of scientificity encouraged ecologists to adopt standard terms, concepts, and methods. Not only was structural analysis common for most ecologists, but also it had the benefit of abstracting out the particulars of a natural system so that it could be reduced to a number of underlying process and forces. Given that most sciences use reductive methods, ecology could align itself with them the more it adopted a similar method. The subject matter of ecology may appear on the surface to require a holistic approach (that is, to understand a natural system, one should study the system as a whole); however, reducing the complexity of the whole to a smaller number of core processes and forces (for example, energy flows, nutrient cycles, food chains and food webs) allowed ecology to purchase more legitimation in the scientific community. In using a structural analysis, ecologists could speak of laws, principles, structures, and could produce diagrams of beautiful symmetry and simplicity.

---

[131] Brown (1941), 63.
[132] Norris (1998), 1.

It was not long after *The Journal of Ecology* began in 1913 that this interest in structural analysis appears. In his transcontinental expedition, Dachnowski noted that "[v]egetation types over large areas in the United States and elsewhere have an astonishing fundamental similarity in regard to structure, differentiation and relation to habitat."[133] What might otherwise seem unique or puzzling was now connected into a more comprehensive explanatory framework. Structure was the link that went deeper into these hidden relations. Through structure, ecologists could see that vegetation units are actually quite similar.

The burgeoning awareness of the importance of structure appears in Canada as early as 1915, when an anonymous reviewer of the first and second reports of the botanical office of the province of British Columbia disparagingly remarked that work was only just beginning and consisted mostly of basic floristic surveys.[134] At this point, ecology resembled little more than the natural history surveys that were descriptive and of little analytical value.[135] A few years later, Klugh proposed an animal classification system based on his work in eastern Canada. This was an important step, he argued, because until then no coherent system existed for classifying animals in Canada.[136] Although Klugh had complimented plant ecologists for having developed such a system in 1918, as late as 1950 Coupland could still note that "[t]he Canadian portion of the great mid-continental grassland of North American is not well known."[137] Having a classification system is but one step in developing a structural analysis of an area. Even with such a system in place for over thirty years, the ecological structure of vast sections of Canada was still poorly known.

---

[133] Dachnowski (1914), 241.

[134] Vegetation of British Columbia; First and second reports of the botanical office of the province of British Columbia (1915).

[135] See Armitage (1918) for an example of a purely descriptive study.

[136] Klugh (1918).

If structural analysis could lead to deeper-level connections between otherwise seemingly disconnected units, then, logically, an even deeper structural analysis would reveal more connections. The quest for deeper-level analysis led ecologists to move beyond mid-level structures and into the more basic ones. Comments that the purpose of analysis was either (1) to look for the "intimate structure of a representative section of … the Grassland Formation of North America,"[138] (2) to obtain information "on the fundamental structure, organization, and interrelationship of … a typical forest,"[139] (3) to discover "the basic structure and workings of flowing water ecosystems,"[140] or (4) to provide "a detailed analysis of the changes in structure and diversity of the plant community"[141] are common in the literature. These types of analysis represent attempts at uncovering what lies beneath the surface. The authors wished to penetrate through to the essence that lies hidden in the details.

The level of abstraction inherent in a structural model such as Lindeman's trophic dynamics encountered resistance from other ecologists at first. Lindeman suggested, in 1942, that natural systems could be described not in terms of what could be seen, but by what could not be seen. In fact, he suggested that the visible details were misleading because they appeared in such astonishing variety that they could be overwhelming.

---

[137] Coupland (1950), 273.
[138] Weaver and Fitzpatrick (1934), 129.
[139] Macnab (1958), 21.
[140] H. Odum (1957), 58.
[141] Woodwell and Rebuck (1967), 53.

Instead, he suggested that the functions of various organisms be grouped together in terms of their use of energy. At the most basic level were producers, who used energy from the sun to meet their needs. The consumers used the producers to meet their needs and so on up the levels. Each level required the level below for survival, but none of these levels could be seen by observation; they had to be inferred. The idea that trophic structure could be used to examine any ecosystem met with staunch resistance from reviewers, one of whom felt that

> a large percentage of the following discussion and argument is based on belief, probability, possibility, assumption and imaginary lakes rather than on *actual* observation and data.... According to our experiences, lakes are *rank individuals* and are *very stubborn* about fitting into mathematical formulae and artificial schemes proposed by man.[142]

Another anonymous reviewer felt "the paper is an essay and papers in Ecology should be research papers. This kind of treatment is premature.... Limnology is not yet ready for generalizations of this kind."[143] In 1942, it seemed ecology was not yet ready to accept such an abstract structural model. Structural models were being proposed, but they were based more closely on observation and less on abstraction. This lack of acceptance would soon change as the power of the trophic dynamic model became more and more apparent.

As structural analyses such as these developed, an awareness grew that treating nature as static would reveal only certain aspects. Examining the structure of a plant community, for instance, could tell an ecologist about that particular structure. From this, she could extrapolate to other similar plant communities in various locations. Furthermore, plant communities change and an analysis of these changes might illustrate a pattern. This could, in turn, provide the beginnings of a structural analysis of change in plant communities. The problem with structural analysis, as Norris notes, is that "[t]he concept of structure … can easily be *immobilized* by assuming it to possess some kind of 'objective' or self-validating status."[144] This danger is perhaps what concerned Lindeman's detractors. They felt that Lindeman's model would diminish the significance of observation because the structure would take on a self-validating role. As a countermeasure, these detractors pressed for continuing close observation that would then suggest its own structure to the ecologist.

---

[142] Golley (1993), ftn. # 15, 213.
[143] Golley (1993), ftn. # 15, 213.

When structural ecologists began searching for the ultimate structure of nature they thought they had found a direct link into the heart of nature. It was felt that with an accurate structural diagram of nature, there would be no distance between the representation of nature and nature-in-itself. However, "[w]hat is suppressed by this static conceptualization is the 'force' or animating pressure of intent which exceeds all the bounds of structure."[145] Ecologists noted this suppression and came to criticize structural models that could not and did not account for change. The developmental aspects of nature also needed to be studied, which required a re-thinking of the structural model. The hope that structural analysis could overcome this shortcoming and, thus, prove to be even more useful is evident in Tansley's comment that "[i]t is now generally admitted by plant ecologists … that the increasing habit of concentrating attention on these changes instead of studying plant communities as if they were static entities is leading to a far deeper insight into the nature of vegetation and the parts it plays in the world."[146] Whether ecologists were examining a static system or a changing one, structural analyses were thought to have the power to probe beneath what ecologists saw as the illusoriness of the surface level.[147]

---

[144] Norris (1998), 50.
[145] Norris (1998), 51.
[146] Tansley (1935), 284.

In the early 1960s, the idea that surface-level analyses can lead to immature or inaccurate conclusions was expressed by Cooper who claimed that "[a]lthough similar in basic composition and structure, the forests at Maverick and at Malay Gap are quite different in appearance."[148] Cooper used the distinction between surface and depth as a warning not to be confused by surface details. He argued that unless the deeper structure and composition was examined, the commonalities lying underneath would escape notice. Heatwole echoed a similar sentiment: "I was impressed with the need for a classification of the litter on a structural basis that would permit comparisons between different areas and vegetational formations."[149] The ability to compare is part of the power of structural analysis—analysis is no longer bound to the specifics and can move freely from one situation to another via the substrate that binds different systems together. The deeper into the substrate the analysis penetrated, the wider its range of applicability. Theoretically, this expansion could continue through larger and larger ranges until every single system in the universe is encompassed within one structure.[150]

Structure binds differences together; but how did ecologists collect the data that they then analyzed from a structural perspective? One favoured method, from the beginning of the 1900s to today, was the quadrat.[151] Ecologists would mark off a square meter of ground, which was then overlaid with a grid such that the exact positions of plants and organic matter could be mapped. These quadrats were then carefully subjected to detailed examination. The quadrat also served another function; by marking off an area, the ecologist was protecting that area from contamination by humans. One had to be careful not to walk through quadrats or drop anything into them. By placing quadrats in various locations, ecologists could efficiently gain a fair representation of a larger area. By adopting the quadrat, ecologists were able to differentiate more quickly one area from another in terms of minute differences, develop norms and evaluative standards, and identify outliers; in short, the quadrat helped to identify otherwise invisible structures in nature and to standardize these structures for comparison. As Golley has said, aggregate data from numerous quadrats within a community allowed ecologists "to observe variations in plant composition that were not visible to the naked eye and created quantitatively defined ecological units."[152] The quadrat was a powerful tool that incorporated structural analysis along with many of the techniques of power/knowledge. Quadrats were used to organize nature (i.e., overlay a grid system on top of nature), provide a method for systematic and detailed examination, and assist in the development of a standardized classification structure that permitted comparison over large geographic scales.

---

[147] Margalef (1963) noted that structural analyses have indicated that ecosystems become more complex and richer over time (that is, the number of components and the relationships between them grow). He suggested the use of the term 'maturity' to evoke this historical development. "Maturity, then, is a quality that increases with time in any undisturbed ecosystem," 358. One is tempted to ask whether Margalef's point could be taken to indicate that humans are immature because we disturb ecosystems.

[148] Cooper (1960), 150.

[149] Heatwole (1961), 267.

[150] For example, see the universal energy model in Odum (1993) Chapters Two and Three. See also Figure 7 in H. Odum (1957), 61. Holling (1992) too noted that "[i]n order to develop policies for sustainable development, a biosphere theory that embraces all scales is essential," 485.

The quadrat was also a visible marker of the distinction between humans and nature. On the inside of the quadrat nature existed in its pure form. Outside the line on the forest floor, the human world dominated. This procedure of partitioning and compartmentalization made it physically possible to mark the boundary between humanity and nature. This line helped define the limits to humanity and also placed limits on where people could walk. Crossing over the line, if done at all, was a carefully controlled procedure that aimed at minimizing the traces left behind by the ecologists on the inside of the quadrat.

---

[151] Priestley (1913) gives an early example of the use and purpose of the quadrat in ecological studies.
[152] Golley (1993), 19.

Returning again to Clements' organic model of the association, des Jardins observed that

> the ecologist is like the physician. Just as the physician studies anatomy and physiology to determine the normal and proper function of the body, the ecologist studies a habitat—temperature range, rainfall, soil conditions, and so onto determine the normal and proper functioning of that area. The ecologist can then diagnose the problems and prescribe treatment to ensure a healthy and balanced organism.[153]

In addition to the power to prescribe and treat, which des Jardins noted, the idea that the ecologist can determine nature's proper functioning is a result of its depth of vision. Foucault discusses a similar effect with medical, surgical science as it positioned itself as more penetrating and insightful than earlier modes of diagnosis. In his *Birth of the Clinic* he stated that a change in the medical profession arose such that 'Where does it hurt?' replaced 'What is the matter with you?' "Foucault's interpretation of this change is to link it with the use of the gaze as a means of establishing a power-relationship between the gazer and the object of the gaze. To look is to assemble information, which combined with knowledge already possessed by the gazer, leads … to the subjection of the subject."[154] The doctor was now able to penetrate to the deeper structural level and see common illnesses and pathologies that the layperson could not. This ability set up an imbalance of power relations between the two whereby the one who gazes objectifies that which is gazed at. Likewise, as the depth of vision increased, the ecologist was able to penetrate to the substrate of the forest or meadow and see the crucial mechanisms at work that were otherwise invisible. Based on this knowledge, the ecologist was able to suggest solutions for what was now seen as unhealthy in nature. Nature, in this scenario, is objectified by the gaze of the ecologist and is produced as a particular type of entity (usually balanced and in equilibrium).

---

[153] des Jardins (1997), 158.
[154] Voase (2005), 322.

Foucault uses this idea of surface and depth to trace a particular history of Western society. According to Foucault, once medicine penetrated into bodies in ways that the vision of the uninitiated could not, it exercised a certain power that normalized those subjected to it. Foucault also used the penal system to illustrate how using disciplinary techniques mapped the body of the criminal and invested in him all the subtle forms of disciplining power that turned the unmarked body into a more fully scribed and inscribable object. This objectification is the docility that disciplining power produces: a docile body primed to adopt and re/produce the norms of society as its own. Through these norms, disciplinary techniques divide the healthy from the unhealthy, the sane from the insane, and the normal from abnormal (criminal). Ecology, with its own version of penetrating vision, divided natural systems into healthy or infirm, and normal or abnormal. A further discursive practice, rejection, accompanies this division of nature. In the operation of ecological discourse, unhealthy natural systems are identified and then rejected as fallen, spoiled, disturbed, or degraded

One of the most powerful norms ecology used to define healthy nature was the steady-state or balanced ecosystem norm. Using this norm, ecologists could determine whether a system was healthy or not. “Perhaps,” the Odum brothers prophesied in 1955, "in the structure of organization of this relatively isolated system man can learn about optima for utilizing sunlight and raw materials, for mankind's great civilization is not in steady state and its relation with nature seems to fluctuate erratically and dangerously."[155] Any system not in balance or steady-state was seen as unhealthy. "The whole structure of any community appears to be designed to make the most efficient use of both the elements and energy. Again, like the successful species, the stable community tends to evolve in the direction of being adept at converting environmental resources into itself and its progeny."[156] Stability and efficiency became the yardsticks for measuring the health of nature.

When ecologists studied agricultural systems and crops, concepts that emphasized stability and efficiency, such as yield, turnover, and production were commonly employed. The aim for most of these studies was to maximize both productivity and sustainability. "The efficiency of the formation of the yield," Clarke argued, "is therefore of great importance to the ecologist, to the conservationist, and to the farmer or fisherman, in order to ascertain whether the actual yield represents a needlessly low utilization or an over-exploitation of the area."[157] The vision of the ecologist penetrated into the crop structure and compared it to the norms of efficiency and stability in order to determine whether a particular level of exploitation was too great or too little. In fact, as Watt discovered, relying on superficial methods such as simple visual counts may produce an inaccurate picture of the health of a population. He concluded

---

[155] Odum and Odum (1955), 291.

[156] Pimentel (1966), 23. Tansley (1939) called systems that were still in the process of changing immature, while ones that are stable were mature.

[157] Clarke (1946), 325. Dambach also recognized, in 1944, that "these interrelationships are essential to the best growth of a forest … [and are] of practical value to the farmer who is interested in getting the maximum return from his woodland" (Dambach 1944, 269).

> that yield may rise steadily as more and more individuals of a critical age group are removed, then crash suddenly. The yield does not warn of an impending crash by flattening out prior to the crash … the first effect of a greatly increased fishing intensity will generally be a surge in yield, even though this fishing intensity can decimate the population.[158]

If we rely on surface level census taking, we may prematurely conclude that current fishing levels are below the maximum limit, when in fact, they are already above a sustainable level. In this example there is no visual warning of the impending crash. It takes the trained eye of the ecologist to assess the structural health of the population.

There are some other specific instances of this deep vision that bear on the discussion. In some studies authors included great tables that detailed total number of species, their ages, ranges, and sex.[159] In other works these great tables were replaced with smaller ones that detailed aspects invisible to the untrained eye. Menhinick's tables, for example, included the oxygen consumption rates at various temperatures for certain species.[160] Tables such as these reflected the deeper view ecologists were adopting throughout the latter half of the 1900s. Whereas the lists' scope was reduced—they looked at fewer variables—their depth was increased.

One can also see the deep vision working when ecologists searched for origins. Stebbins and Major, for instance, quoted Braun-Blanquet who in 1923 said that

> [t]he study and precise interpretation of the endemism [i.e., native species and populations] of a territory constitute the supreme criterion, indispensable for arriving at any conclusions regarding the origin and age of its plant population. It enables us better to understand the past and the transformations that have taken place. It also provides us with a means of evaluating the extent of these transformations, the approximate epoch

---

[158] Watt (1955), 288.
[159] See Dirks-Edmunds (1947) for one example and Leopold and Jones (1947) for another example with a fifteen-page table containing 328 entries.
[160] Menhinick (1967).

> when they occurred, and the effects which they produced on the development of the flora and the vegetation.[161]

No wonder ecologists sought origins. The concept of origin was a powerful one. It allowed for a discussion of age, the past, any transformations, as well as evaluations of them and their effects. The quest for origins was the quest to get below the surface of things, to see what was really there to be seen by the trained, penetrating eye of the ecologist. Without this training, we could be duped into thinking that plants (or animals) had a different history and, therefore, we would think their future differently (incorrectly) as well.

Continuous attempts to monitor animals are another type of study ecologists have carried out that clearly show this depth of vision. Savage, for example, was a pioneer in using remote sensors to monitor animals. In his experiment, he had to overcome a number of technological hurdles: "the sensor must put out an effective signal for the period of the experiment, must stay in place, must not cause undue curtailment of the animal's functions, and the connections from the sensor to the 'outside world' must not cause irritation or infection." These challenges were well worth the effort because "[t]he concept of transmission to the surface of the body and subsequent relay by transmission is very attractive…."[162] With the sensor in place, Savage could monitor not only the location of the animals, but also the rate of movement and temperature. He dreamed of the day, not too far in the future, when he would be able to monitor oxygen and carbon-dioxide variables, energy intake and expenditure, and even telestimulation and automatic control of experimental variables. Ecology had now penetrated the very body of animals. The power to see and monitor makes the shift from a position external to living bodies or internal to non-living ones (dissection for instance) to live, real-time, internal (otherwise invisible) processes inside animal bodies. This process continues with more recent advancements in monitoring technology such as the GPS, infrared tagging, remote sensors, and more sophisticated and detailed electrode attachments and scans..

---

[161] Stebbins and Major (1965), 3. See also Cavers (1914) and Meyer (1937).

[162] Savage (1966), 86-87. It is interesting to note that, in his excitement, Savage downplayed the challenge of maintaining the animal's welfare. He mentions only protection from infection and irritation, which are at least equally a problem for the experimenter as for the animal. His concern is for *undue* curtailment and stress; outside this, curtailment and stress are not a problem.

This chapter outlined some of the elements in the history of ecology that are representative of the rules governing the formation of statements, objects, theories, and models in ecological literature. As a science, ecology sought to produce knowledge that conformed to certain strictures. In the main, knowledge had to (1) be progressively developmental (and show that nature itself was progressively developmental), (2) be as universal and standardized as possible, and (3) penetrate further and deeper into nature to unravel its secret structure. These requirements are indicative of the various thresholds through which ecology passed. They also reflect the techniques of discipline that characterize the modern power/knowledge nexus (that is, surveillance, normalization and standardization, and differentiation and classification).

The rules of formation and the discursive practices of ecology are linked to the discourse in OR, particularly that on minimum impact. This linkage is the subject of the next chapter where I outline the practices and rules governing the production of knowledge in OR. Specifically, I show how these modernist disciplinary techniques manifest in OR. For example, differentiation and classification appear in OR in the conceptual attempts to organise wilderness areas according to criteria such as a lack of human-built structures, types of intact ecological relationships (for example, nutrient cycling in water sources), and the frequency of intrapersonal and intra-group encounters. Furthermore, understanding the discursive practices of ecology and OR enables a discussion and analysis of the effects these rules have had on our conceptualization of nature and thus our subsequent treatment of it.

CHAPTER 3: THE STRUCTURE OF OUTDOOR RECREATION

The clearest way into the universe
is through a forest wilderness. ~ John Muir

After discussing the discursive formation and practices of ecology in the previous chapter, I now turn to the relationship between the power/knowledge nexus of the modern disciplinary regime and the field of outdoor recreation. Specifically, I am interested in the ways outdoor recreation embodies, in a manner that is both the same and different from ecology, some of the techniques of the power/knowledge regime. These techniques revolve around three central and interrelated issues in OR. First, there is the debate over freedom and regulations. Protecting wilderness, OR has argued, often requires regulations on behaviours and the setting limits on numbers of users. However, these restrictions run counter to the idea that wilderness experiences should embody freedom: freedom of choice, freedom of movement, freedom from externally enforced rules, and freedom from social norms and expectations.[1] The tension between freedom and regulation has been a focal point for much discussion as OR researchers sought a way to protect wild areas while at the same time allowing for the benefits these areas could provide users.

There is a long history in Canada of promoting wilderness use as an activity with various attributes and benefits. Early proponents of outdoor activities in the 1880s argued that urban environments, while positive in many regards, had a number of negative effects on people. For example, modern civilized life was soft compared to the rigours of roughing it in the wild. People argued that getting out into nature and leaving behind the luxuries of city life strengthened one's body, mind, and soul. Health and nature were often connected: certain ills of society could be alleviated by taking time off from the hectic and stressful pressures of life in a city by canoeing for a week in the wilderness. Early national park proponents felt the same about the curative powers of the hot springs and thus promoted Banff as a rest cure for people needing to restore their health and peace of mind.[2] Historian Robert Brown contended that early park developers and politicians saw national parks as resorts more than as wilderness. The idea of amenities and services was a common conceptualization of parks until the First World War. Brown called this the doctrine of usefulness. Unused land was a waste and Canada's west had more than its share of unused land. Hence, Prime Minister Sir John A. Macdonald could see a scenic range and a gold mine in the same way: because they were useful, they were both beautiful and should be encouraged as much as possible.[3] The attitude of the time (1885-1914) was one of utilitarian use of the curative powers of nature: "[t]he government thought it was of great importance that all this section of country [Banff springs area] should be brought at once into usefulness."[4]

---

[1] See, for example, Christensen and Cole (2000); Cole (2002); Ewert (1995); Harkin (1957); McCarthy and Dower (1979); Marsh (1978); Newman et al. (2003); Newman et al. (2005); and Watson (2004).

In a linked manner, the usefulness of wilderness was seen as curative in that it strengthened bodies, cleared minds, and re-connected people with God in the form of nature's awe and beauty. This curative ability was sometimes promoted as rest cures in a canoe. Both Banff hot springs and rest cures in a canoe were seen as deriving their curative powers in part from their distance from the city. Free from the stress and other ailments caused by city living, canoeing and national parks offered not just an escape but also a chance to heal precisely because people were free from the city. J. B. Harkin, known as the father of the national parks in Canada for his role in creating our national park system, believed that "parks represented the moral value of outdoor recreation, a haven from degenerate cities."[5]

---

[2] See Jasen (1995).

[3] Brown (1970). See also Loo (2006) who suggested that "[m]anaging wildlife in the parks was not simply about protection: it also involved deploying wild animals to enhance and indeed create a generalized wilderness experience for tourists," 27. In short, wildlife needed to be useful to be valuable. MacLaren (1999) and Nelson (1982) give a brief history and discussion of the early formulations of parks in Canada that includes the idea of usefulness as a guiding principle.

[4] Brown (1970), 49.

Similar to this was the notion of the value of fresh, clean air to breathe. As Canada's population became more and more urbanized in the early twentieth century, the feeling grew that pollution, crowding, and the fast-pace lifestyle of cities were compromising the health of people. The Canadian Commission of Conservation, established in 1909 by the federal government, wrote in its annual report of 1914 that "[i]ndustrial smoke disfigures buildings, impairs the health of the population, renders the city filthy, destroys any beauty with which it may naturally be endowed and tends … to make it a squalid and undesirable place of residence."[6] Not surprisingly, an antimodern movement developed in reaction to the pressures and threats of modern civilization. The antimodern movement was broad ranging and shifting. Wright described it thus:

> [i]n opposition to modernity—its unbearable lightness, anonymity, and its awesome capacity to obliterate tradition—antimodernism venerated, in its various guises and forms, the idealized premodern community of face-to-face contact, rootedness and connection. Essentializing things agrarian, traditional, ordered and unencumbered by modern complexity, contradiction and transience, antimodernism promised authenticity in a world of simulacra and offered therapeutic tonic to the unease of modern life.[7]

In its more radical forms, antimodernism encouraged people to leave the city entirely and live life out in the country, as many of the previous generations had done. The ills and risks of city life placed more than just physical and even emotional health at risk. City life was also threatening to middle-class values. One specific antimodern desire was "to toughen a flabby bourgeoisie against the threat of anarchists, immigrants, strikers, tramps and criminals" while at the same time preserving the legacy of individualism.[8] Antimodern literature confronted many perceived threats to middle-class concerns, such as "idleness, irresolution, avarice and other moral shortcomings…."[9] The antimodern movement saw nature as the remedy or tonic for all these ills.[10]

---

[5] Quoted in MacLaren (1999), 41.

[6] Quoted in Francis, Jones, and Smith (2008), 153.

[7] Wright (1997), paragraph 2.

In Victorian Canada, a popular view of nature was adduced as evidence of God's presence and influence in the world.[11] The more time people spent away from nature, the greater was the potential for losing this connection with God. For the first time, a whole generation in Canada was being born and raised mainly within an urban environment. Many cities experienced incredible growth in the early 1900s. For instance, Montreal and Toronto nearly tripled in size between 1890 and 1920; Winnipeg grew seven-fold during the same period, while Vancouver increased twelve-fold. Calgary's population exploded (a sixteen-fold increase). Between 1901 and 1911 Canada's population increased sixty-three percent; most of this growth occurred in cities.[12] Growth at this rate had never happened in Canada's history and fears arose concerning the effects this growth would have on people's moral health. Muscular Christianity arose as an attempt to off-set the lack of contact with God through nature. "Among theologians," Conrad and Finkel have suggested, "it became fashionable to refer to nature as a medium whereby people could communicate with God."[13] Theologians such as these and Muscular Christians supported the linkage between physically active lifestyles and moral development. Fears of moral degeneration, laziness, and a more general deterioration of the social fabric motivated Muscular Christians to seek out more active lifestyles in keeping with God's design for humans. Ideas of hard honest work and participation in physical sports leading to improved moral development, closer ties with God, and better physical health were cornerstones of the Muscular Christian movement. Thus, to live in the city and to work in a predominantly sedentary occupation meant jeopardizing one's relationship with God.

---

[8] Lears, quoted in Dawson (1997), paragraph 9.

[9] Lears, quoted in (Dawson 1997), paragraph 9.

[10] Altmeyer (1995) discusses three views of nature in Canada during this time. The first one he discusses is nature as benevolent mother, which is essentially the view that nature eases the ills of the city.

[11] See Berger (1983); Chapter Four: 'The Veils of Isis,' in McKillop (1979), 93-134; and Waiser (1989).

[12] See Francis, Jones, and Smith (2008), 137.

[13] Conrad and Finkel (2003), 342.

The idea that nature was curative or healthier than city life remained a central conceptualization of wilderness for most of the twentieth century. Whether or not it was connected to religion, nature was set apart from urban life and the distance between them became a measure of the value of wilderness: the farther away these areas were, the freer city-people could be in them. As outdoor recreation became a professional field in the 1960s, the idea that the value and role of outdoor recreation in Canadian society lay in the increase in personal freedom along with hard honest work as a means for improving both individuals and society was beginning to divest itself from the simple binary opposition between city life and roughing it in the bush. Suggestions that one could cure the ills of society by escaping in a canoe for a week were now complicated by the growing realisation that the effects of the city may be more entrenched and widespread. The idea that leaving the city behind meant returning to a healthier natural state in which the artifice of the city played a much reduced role and people could recuperate was still promoted; however, as cities grew and their influence spread, two consequences resulted. First, there was the two-sided problem of vastly increasing urban populations coupled with the reduction in wild places into which people could escape. Second, the expanding city populations were having a greater and greater effect over a larger and larger area outside their corporate limits. Pollution, alterations to river flows due to dam construction, and garbage disposal numbered among the problems that began to exert more and more pressure on wilderness areas. If living in the city subjected one to these ills, getting out into pristine wilderness areas that were exempt from these ills was growing more difficult.

The connections and tensions between the freedom that wilderness provides and the regulations deemed necessary to protect that very freedom form the first site for the deployment of the power/knowledge regime in OR that I examine in this chapter. As in the previous chapter, so here the purpose is not to outline every contour of the debate, but rather to look behind the particulars and examine the surface upon which such a debate was framed and the discursive practices used in the debate to form positions and construct arguments. The second site I examine in this chapter is the discourse on wilderness-as-authentic-nature and the difficulties this presents vis-à-vis the notion of change. OR literature promoted the use of wilderness areas for a variety of reasons. One of the concomitant effects of successfully promoting wilderness use was an increase in the number of users. Along with more people came more impacts. Wilderness areas were changing in many ways; development was taking place in many areas as amenities and different facilities were established. Even in the areas with little or no development, the sheer number of people passing through them (on foot, on skis, on motorized off-road vehicles, or in boats) was having a serious effect.

Grappling with the conflicting desires to promote wilderness use while keeping these areas wild was becoming increasingly both more difficult and more important and pressing. As a result, much of the literature in outdoor recreation engaged with questions of just what kinds and amounts of change were acceptable in wilderness areas. The legacy of rest cures, Muscular Christianity, and antimodernist thought had positioned wilderness areas as qualitatively different from urban spaces. It was the very difference of these areas that offered important benefits and services that cities just could not provide. As these areas were changed by encroaching urbanization, however, concerns grew over their ability to provide those services and benefits.

The notion that wild areas were special was tied to their geographical, geological, and ecological traits. While these were at first understood as either self-evident, or simply the opposite of urban characteristics, as the field of OR emerged in the 1960s as its own entity (that is, it crossed the threshold of positivity), the distinction between urban and wild places became more complex. Some wild places, like national parks, had existed for nearly a century and were accommodating more and more visitors every year. It was impossible not to notice the changes these areas were undergoing. Of particular concern in OR were changes in the fundamental ecological characteristics of these areas. When an area underwent dramatic ecological changes (such as when mining operations occurred in protected areas or hotels and shops were built to accommodate visitors), the wilderness benefits that that area could provide were threatened. As evidence of changes that threatened natural areas grew, OR scholars were faced with the challenge of not only specifying what benefits were being threatened (as well as why and how they were threatened) but also of identifying exactly the characteristics of wild areas that provided those benefits. The further into these issues OR researchers went, the more complex the challenges became. By the 1980s, determining what exactly it was about wilderness that was so valuable to people and even what exactly a wilderness area was became entangled with the realisation that these areas would not last if left on their own. In other words, changes were taking place that threatened not just one natural area or the appearance of natural areas. Changes were occurring that had the potential to eradicate all wilderness in Canada. This potential was particularly problematic because the definition of wilderness framed natural areas as places that could/should be left alone to develop on their own. The conflict between defining wilderness as areas that change on their own accord and the growing realisation that urban influences were expanding such that no wild area would change only on its own produced the catch-22 that OR scholars have had to grapple with since the 1980s: wilderness was defined as land that changed naturally, even if humans had to manage those changes to ensure they happened naturally.

However, it proved much more difficult to reconcile changes in wilderness areas with OR's understanding of what those areas were. It is at this point where the challenge of identifying the central characteristics of wilderness and protecting those traits (and increasingly to re-establish them) collided with the idea of change and became a central problematic in OR discourse. A discussion of this point and OR's response to the challenges and fears surrounding change and authenticity offers insight into another approach to the seeming impasse that OR has encountered between the inevitable changes that wilderness is undergoing and the desire to keep protected areas free from change so that they can exist in their pristine and natural state. The ideas, challenges, fears, and desires surrounding notions of change and authenticity form the second site where the discursive practices of OR become evident.

The final site I examine concerns the atomic, independent self that modernity produced and privileged. As a discipline that developed in the late twentieth century, OR exemplifies this modern conceptualization of the self. The literature on the benefits of, motivations for, and constraints on outdoor recreation, as well as the discussion about the wilderness experience, are illustrative of the modern image of the self. As this literature developed, the benefits derived from and the motivations behind backcountry use grew in sophistication and number. Researchers recognized, for instance, that what people preferred was not always ecologically sound. Campsites are one indication of this gap. Overall, users prefer denuded campsites with hard-packed ground to sleep on. A denuded site may make the perfect campsite, but it hardly exemplifies the perfect ecosystem.[14] The difference between what motivated people to use the backcountry and what was deemed healthy for that land complicated the OR field as it realised that some of the benefits and experiences sought in the backcountry may turn out not to be good for that country.

The understanding of the benefits of and motivations for wilderness use changed from earlier formulations of a simplistic binary distinction between cities and wilderness to more complex and sophisticated understandings that recognized that simply being in the wilderness would not necessarily lead to positive outcomes (or not necessarily positive for everyone or equally positive for everyone). Researchers recognized that it was not enough to just leave the city behind and head out into nature. Many interacting forces, including gender, class, age, socio-economic status, ethnicity, and ability, played an important role in both the type and number of benefits one could derive from a wilderness experience. While the literature on benefits and motivations has become far more sophisticated and nuanced, it has continued from the 1960s onward to rest on, require, and help produce the modern image of the self-contained self. For example, over the years many scholars have written about such benefits as the increased self-reliance, strengthened self-efficacy, and improved self-image that can result from wilderness experiences.[15] These are the kinds of benefits that apply to individuals. There were other benefits, as well, such as socialization, the development of pro-social behaviour, increased ability to co-operate and co-ordinate, and improved communication skills all of which researchers suggested wilderness experiences had the potential to develop. Nevertheless, even these more social benefits did not contradict the dominant modern view of self-hood and in fact can be seen as supporting it because it was still individuals who interacted, communicated, and co-operated. There is a difference in being alone in the wilderness and being an individual in it. The literature on benefits of wilderness use speaks of both solo and group wilderness use, but focuses almost exclusively on individualistic benefits for either use type. Even benefits such as civic benefits (for example, an increased sense of civic duty and the development of citizens), while also not directly conceptualized as individualistic, were premised on individuals becoming citizens. Overall, this literature grew in sophistication over the years, and showed that many factors interacted to produce (or not) the benefits of wilderness experiences.[16] The third site I examine is this literature on benefits and motivations, in which the techniques of power/knowledge were targeted at individualizing each user as a unique and independent entity.

---

[14] See Farrell, Hall, and White (2001) and White, Hall, and Farrell (2001).

---

[15] See, for example, Klint (1990); Chapter Six 'Motivations for Recreation: A Behavioral Approach' in Manning (1999), 79-95; and Chapter Four 'Individual Behavior and Motivation' in Priest and Gass (1997), 41-60.

[16] It should be noted that not all the literature was unquestioningly positive about the benefits of OR. Some authors warned, for instance, that gender issues had the potential to turn a positive wilderness experience negative for some. This is an important component of the literature on benefits (see, for example [Miranda and Yerkes (1987)] and [Warren (1990)]); nonetheless, it did not contradict the modern image of the self.

These three issues (debates on freedom versus regulation, challenges over change and authenticity, and the benefits of and motivations for wilderness experience) illustrate some of the tactics and strategies by which the modern power/knowledge regime influenced what objects of knowledge were produced (and how they were produced) in OR. Understanding how OR frames discussions of regulations versus freedom, changes to authentic natural areas, and the benefits they provide to users is the first step in being able to offer alternative ways to think about nature and our relationship with it. In particular, one aspect that clearly arises as a result of examining these three sites is the question of paradoxes. The paradoxical character of many of these debates has been commented on by numerous scholars. The tensions between encouraging more and more people to use the backcountry while at the same time trying to protect those areas from excessive impacts from that use is one such paradox apparent in the writing on minimum impact. Scholars and researchers have struggled with this issue of paradoxes for over forty years. For the most part, these struggles have centred on ways to resolve the paradoxes (for example, finding a way to limit impacts so that more people can enjoy the wilderness would resolve the above paradox). To date, no such solution has been found that would satisfy both sides of the paradoxes. Progress has certainly been made; however, the nature of paradoxes is that their solution requires the elimination of one of the poles in favour of the other. So, to allow an unlimited amount of freedom in wilderness would require a complete elimination of all regulations, which is, of course, impossible. As this level of elimination is an untenable and extreme position, OR scholars have opted for a compromise between use and protection, freedom and regulation, and change and authenticity. Compromises, however, operate within the same structure as paradoxes: instead of one side winning, both sides remain locked together in tension each giving and taking from the other. The paradox still remains. Compromises change the intensity of the tension with the hope of eliminating it or reducing it to a bearable level, but the tension remains. In short, through compromises we can approach the mythical resolution of paradoxes, but we can never arrive.

This is similar to the paradox of the frog: if a frog wants to cross to the other side of a lily pad, it might be able to jump half the distance. Suppose it halves the distance with each subsequent jump. No matter the number of jumps, it will never be able to reach the other side because it always only takes half of the remaining distance, never all of it. In a similar fashion, OR researchers have gotten closer and closer to resolving paradoxes, but no matter how many times they move half-way toward the solution, the solution always remains just out of reach. I contend that the inability to reach a solution is not a result of poor research or faulty conclusions, but is, rather, the essence of paradoxes: they are irresolvable. The purpose of this chapter, in relation to paradoxes in OR, is to show the unending progress that has occurred (and likely will continue to occur) in the field with regard to the two paradoxes of freedom/regulation and change/authenticity. Without undermining the productivity of the research addressing paradoxes in OR, I wish to suggest another means for dealing with paradoxes that would open new avenues for research.

A second purpose for this chapter is to illustrate a fundamental structure upon which the field bases much of its research: the modern self-contained individual. It is important to understand this structure for a couple of reasons. First, the complex of assumptions and requirements surrounding the modern self forms one of the most influential organizing structures in outdoor recreation discourse today. Understanding this organizing structure in OR illustrates a sizable portion of its discursive practices. Furthermore, the rules that govern the production of knowledge on benefits and motivations are themselves products of a deeper discursive practice that divides humans from nature. OR is, of course, not the only discipline to rely on and strengthen the modern conceptualization of the self. Sciences, such as ecology, also engage this image of an independent self that interacts with an exterior world. That is, the modern self is seen as separate from the world. Individuals who participate in OR activities interact with the natural world, but are not the same as that world. In fact, a significant portion of the benefits derived from OR stem precisely from this separation. Thus, for instance, without the distinction between nature and humans, there could be no benefits in visiting the natural world. I am not suggesting that OR begin to conceptualize humans and nature as the same. Instead, I am arguing that conceptualizing humans and nature as separate, while valuable in some regards, is limiting in other ways. Maintaining a sharp distinction between what is natural and what is artificial is itself a paradox, and thus cannot be resolved in any simple manner. This particular paradox, unlike the other two mentioned above, has largely escaped comment by researchers in OR. The second purpose of this chapter is to highlight this paradox visible in the image of the modern self and prepare the way for a discussion of an alternative approach to dealing with paradoxes in general.

A third and final purpose for this chapter is to highlight the commonalities and distinctions between ecology and OR. In Chapter Two I discussed the ways that ecological discourse is organized and its objects of knowledge produced. In many ways, the structure of ecology has remained constant; that is, its archive changed but little.

However, the types of statements and concepts enunciated within ecology did alter fairly significantly in the mid- to late-1970s. Not so for outdoor recreation. Ecological discourse may have undergone significant change, but OR has remained committed to the older objects and statements ecology produced. This parting becomes a central component of the alternative that is developed in Chapter Eight. This current chapter offers a genealogy of the discursive practices in OR in order to show where it borrows from ecology's rules of production as well as where ecology differs in important ways that OR would benefit from were it to adopt some of the newer theories in ecological discourse.

The Disciplinary Structure of OR

> *I'd like to share a revelation that I've had during my time here. It came to me when I tried to classify your species; I realised that you're not actually a mammal. Every mammal on this planet instinctively develops a natural equilibrium with the surrounding environment, but you humans do not. You move to an area and multiply and multiply until every resource is consumed. The only way you can survive is to spread to another area. There is another organism on this planet that follows the same pattern. Do you know what it is? A virus. Human beings are a disease, a cancer of this planet. You are a plague and we are the cure." ~ Agent Smith, The Matrix*

For the purposes of this work, I take outdoor recreation to refer to the professionalization of the field that began in the 1960s in the USA. In terms of Canadian research, the field developed more slowly and adopted much of the scholarship produced in the United States. The first professional journal in North America dedicated to leisure, *The Journal of Leisure Research*, began in Virginia in 1969 and marks one moment in the professionalization of the field. University programs also began developing in the States throughout the late-1960s and early-1970s.[17] Early textbooks on OR began to appear in the late-1970s/early-1980s.[18] Another prominent journal, *Leisure Sciences* (New York), produced a special theme issue in 1981 on outdoor recreation in parks.[19] Although the topic of outdoor activities has been the subject of writing in other fields (for example, scholars wrote about national parks in both Canada and the USA throughout the 1900s), it was not until the 1960s that a separate field of outdoor recreation scholarship really came into its own. In Canada, the development of a distinct OR field occurred slowly and drew heavily on the pre-existing scholarship in the USA. As a result, although I use Canadian examples wherever possible, in many cases, especially those preceding the 1980s, there were few Canadian perspectives and those that existed followed a similar structure as studies in the United States. So, while there are definite differences between the history of Canada's protected areas and those of the United States, in terms of recreation scholarship, the differences are less pronounced. In tracing the background history that led to the emergence of OR as its own field, I have focused on a Canadian perspective, but in terms of OR as a discipline, I treat the field as largely homogeneous over North America. There are, however, significant differences between North America and Europe that are the result of differences in land use patterns and historical factors.[20] Within North America, on the other hand, the similarities in OR scholarship between Canada and the USA are greater than any connections to European outdoor recreation literature. Consequently, I focus almost exclusively on North American OR scholarship.

---

[17] Simmons (1985).

[18] Dustin (1985), Jubenville (1976), Martin and Inglis (1984), van Doren, Priddle, and Lewis (1979), and Wall and Marsh (1982).

[19] Field (1981)

[20] Swinnerton (1995).

Considering outdoor recreation, we see that the techniques for the production of knowledge manifest differently than in ecology. In OR, the techniques of surveillance, for instance, do not place as much emphasis on the quadrat; instead, they can be seen more in issues such as freedom/constraints and change/authenticity. As we will see, the literature on outdoor recreation has promoted freedom from constraints; yet, at the same time, it has stressed regulations in order to protect the wilderness from excessive use. Increasingly since the 1970s, these regulations appear in the form of minimum impact requirements for all backcountry users; however, there have been other regulations such as use limits, group-size limits, and behavioural prohibitions. The emphasis on freedom/regulations and change/authenticity required compliance from users. That is, the more people violated the regulations or the more wilderness areas changed as a result of human use, the more wilderness quality and integrity were threatened. As a result, OR discourse deployed certain tactics that were designed to improve compliance by creating situations where users could be watched (or could feel the potential for being watched) for infractions, where standards and norms of behaviour were constructed and disseminated, and where wilderness uses were differentiated and organised into good or acceptable and negative or unacceptable.

As we saw in the last chapter, ecology employed the tactics of division and rejection along with differentiation and classification as important elements in the organization, production, legitimation, and verification of knowledge. In OR, meanwhile, the same kinds of techniques were deployed, but their targets and aims differed. In OR, for example, classification of nature often followed the template used in ecology (species, communities, associations, and so forth) but was also applied in different ways. In OR, classification and division were also directed at distinguishing types of impacts and types of users. Coupled with the potential for surveillance that OR also promoted, the tactics and strategies of classification and rejection were designed to create more compliant, self-disciplined, and docile users in the hopes of protecting the type of landscape central to the entire field.

These techniques became more refined over time and were applied in a more tightly focused manner with greater amplitude to activities surrounding wilderness areas in Canada as the discourse on minimum impact began growing in the 1970s. In the early decades of the 1900s a common conceptualization of Canada (especially Western Canada) was a vast and untamed landscape with nearly unlimited resources.[21] This view enabled those who promoted tourism in Canada to do so with less thought toward the consumptive side of tourism and outdoor use. As a result, some felt that restraints on numbers or uses were of limited importance because the chance of damaging the vast amount of wilderness in Canada was seen as slim. J. B. Harkin, for instance, declared that outdoor tourism should be promoted because while "the tourist left large sums of money in the country, he took away with him nothing that left the country poorer."[22] At this time, outdoor tourism was seen to have little impact on nature: it was mainly a benign or non-consumptive industry. As the century wore on, however, it became increasingly difficult to maintain this perspective. Not only were ideas and attitudes toward development changing, but so too were understandings of outdoor recreation. After the 1960s, unrestrained tourism was seen more and more as a non-conforming or at least a problematic use of wilderness areas. This change gave rise to the problem of how to balance use with protection.[23]

---

[21] Altmeyer (1995) indicates that one common view of nature was a storehouse of resources. This storehouse was so vast that few were concerned with depleting it.
[22] Harkin (1957), 8.

Understandings of the role and value of wilderness in Canada have changed throughout the twentieth century. At first, industrial activities in wilderness in Canada were seen as necessary and good: logging and mining were permitted, even encouraged in parks. At the same time, wilderness and other protected areas in Canada were designed to accommodate visitors, which required further infrastructural development: rail and road development, lodgings, and other amenities. Until the third decade of the 1900s, wilderness areas were seen as storehouses of natural resources and resorts for (wealthy) tourists. Then, in 1930, the national park act stipulated that development in national parks should be evaluated in terms of the effects it would have on the environment. This criterion, however, was equally about preserving the environment for people to enjoy as it was about preserving the environment for itself and has become the (in)famous dual mandate of parks in Canada: protection with use. As a result of the dual mandate, parks and other protected areas began establishing guidelines that classified certain activities as non-conforming to their goal of protection and enjoyment. Thus, the tension between use and protection persisted even as the terms of the debate shifted.

Killan chronicled the rise of environmentalism and its impacts on park policy, designation, and management in Ontario. He noted that although tourism was originally seen as compatible with parks, by the 1960s this view was changing. The debates surrounding the purpose of parks and what constituted non-conforming uses began to include not just mining, logging, and other extractive uses. Sport hunting, fly-in fishing, motorboat use outside of access points, and commercial tourism had become debatable uses of parks and protected areas.[24] In sum, as the growing environmental movement spread and the numbers of backcountry users increased, more people began to see that the use of wilderness for recreational purposes was not benign. Consequently, the pressure of outdoor use on the land was increasingly being commented on. As Morrison noted, even as early as the mid-point in the twentieth century, park use in Ontario had reached such a high level that planners were scrambling to meet the demand. At this point, planners and government officials began to comment on the increasing impacts of park use.[25] Harkin's own viewed also changed over time: "[m]an can maim, disfigure, and weaken Nature, but once he has destroyed original conditions, he can never replace them."[26] The view that recreation and tourism were non-consumptive was being challenged. Wilderness (over)use could indeed threaten the very qualities that defined the wilderness.

---

[23] See Priddle (1978) who suggested that increased use of the backcountry in Ontario's provincial parks was recognized by the late-1960s as damaging. As a result, "[s]uddenly, parks personnel [were] put in the unenviable position of deciding who can go where, to do what, and how they can get there," 218.

By the 1970s, concern for the environment was becoming a more central component in Canadian society and this was reflected in OR discourse. The first Earth Day, in 1970, is often regarded as a landmark moment when concern for the environment galvanized.[27] Not surprisingly, outdoor recreation's emphasis on minimum impact began in the 1970s, as well. Early studies on the impacts of backcountry recreation shared some characteristics with ecological science. Beringer, for example, commented that in adventure therapy most models follow the dominant paradigms of science.[28] As we saw, one of the major aspects of this scientific paradigm in ecology was the emphasis on quantitative models. In OR a similar emphasis can be seen.[29] Wilkinson, for instance, lamented that knowledge of the relationships between recreation and the environment is poor. He acknowledged that the level of knowledge was due to the lack of an overall analytical framework. Furthermore, he said there are "no commonly accepted quantitative models that can accurately measure the impacts of particular recreation activities on specific environments."[30] The reason why quantitative models were stressed in OR and ecology relates to their predictive abilities. Along with prediction, control (a common theme in science since the 1600s) was another element in outdoor recreation literature. In particular, prediction manifested as attempts to predict what kinds of experiences or attitudes will have the greatest likelihood of instilling environmentally responsible behaviour in backcountry users. If researchers could determine what best predicted this, they could design wilderness experiences and programs to prompt people to develop environmentally responsible behaviours and attitudes. The result is that, through prediction, people's future behaviour could be controlled (or at least influenced). This behaviour was divided and classified according to the impacts it had on nature. Using basic ecological concepts such as species diversity, the disciplinary techniques in OR discourse were targeted at influencing those actions that had been classified as threatening or damaging to nature.[31]

---

[24] Killan (1998).
[25] Morrison (1982).
[26] Harkin (1957), 13.
[27] See for example, Worster (1994).
[28] Beringer (2004).

As with the previous chapter on ecology, this history of OR discourse is not intended to be comprehensive. Instead, I am restricting the discussion to issues that deal with environmental quality; ecology, nature, or the environment; minimum- or low-impact techniques; or the overall purpose of outdoor recreation. OR in general may well be quite similar to science in general; nonetheless, I consider only certain works. Other aspects of OR literature, such as studies on economic costs and benefits are part of OR's discourse but are not specifically connected with the issue of impact and the environment and thus will not be taken up in this chapter. The temporal dimension of this history spans only the past forty to fifty years (since circa 1960): ecology's spanned over ninety years. In addition to the length of time, there is another significant difference between this chapter and the last. In discussing ecology, I highlighted the layers of practice that produced the objects of ecological science. Of importance there were the connections between the discursive practices and creation of knowledge. The links of descent that connect those practices with earlier ones in natural history and pre-1880s science played a less central role. The present chapter, by contrast, is concerned precisely with issues of descent. In this way, Chapter Two employed more of an archaeological framework to sift through layers without making an overt attempt to connect them to earlier practices. Chapter Three employs more of a genealogical framework in that I am concerned specifically with the connections in OR that descended from ecological and scientific discourse.

---

[29] There are, of course, many variations on the kinds of research done in OR. In relation to the environment and minimum impact studies, there has been a move in OR toward more quantitative studies that could produce 'hard' data. Anecdotal and qualitative studies have their place, but certain researchers have expressed a need for more quantitative ones.

[30] Wilkinson (1992), 181.

[31] See for example Cooksey, Dickinson, and Loomis (1982), Cordell, Betz, and Green (2002), Ewert, Place, and Sibthorp (2005), Hughes and Estes (2005), Shafer, Hamilton Jr., and Schmidt (1969), Tarrant and Green (1999), Thapa and Graefe (2003), and Wilkinson (2003).

The differences between these two chapters should not eclipse the fact that neither is a narrative history of the changes in either field.[32] For Foucault, neither archaeology nor genealogy attempt to produce a continuous flow of history. In fact, Foucault often remarks that it is the inconsistencies and gaps that interest him more. In a similar fashion,

---

[32] The distinction between Chapter Two and Three as archaeology and genealogy, respectively, is also not meant as a comprehensive distinction. That is, there are elements of both frameworks in each chapter.

I trace the connections of descent between ecology and OR, but I also trace the breaks in descent in places where one might expect links. For example, some ecological concepts appear in OR literature, but there are also many concepts that do not appear or that appear in very different configurations in OR discourse. These instances where objects in ecology that would seem at first to be applicable in OR but are not actually present are important because they reveal another dimension of the discursive practice in OR. Discursive practices produce objects of knowledge, but they also restrict the production of objects: not everything is permitted. Attending to the gaps highlights those practices that limit, circumscribe, or prohibit and are thus important points to consider.

You Can't Have Your Cake and Eat It Too: Freedom and Regulation in Wilderness

> *The most important issue we have to deal with is*
> *freedom of movement. ~ Anna Lindh*
>
> *Why has it seemed that the only way to protect the environment is*
> *with heavy-handed government regulation? ~ Gale Norton*

Most of the OR literature related to minimum impact and environmental issues places the desire for freedom and choice against the need for restrictions and regulations.[33] On the one hand, wilderness areas are supposed to be free from artificial constraints. In 1918, a Canadian woodcraftsman, Nessmuk, commented on why people go camping in more primitive sites: "[w]e do not go to the woods to rough it; we go to smooth it. We get it rough enough in town. But let us live the simple, natural life in the woods and leave all frills behind."[34] Nessmuk was speaking of a life in the out-of-doors and exemplified some of the antimodern and back-to-nature sentiments of his time. The idea that nature can offer a refuge from city life, however, was carried into the development of modern camping and outdoor recreation in Canada. Much of the literature in OR emphasized that to be outdoors was to be outside arbitrary cultural injunctions. Life in the wilderness was seen as simpler, purer, and cleaner (physically and mentally). This thinking led to the belief that we "travel over familiar routes at home, with work to be done along the way; while in outdoor space, we explore and go wherever we wish without obligation."[35] Leaving behind the frills of city life and entering wilderness areas equated to a type of freedom or escape. This equation strengthened the belief that wildland "recreation and regulations are inherently contradictory because freedom and spontaneity lie at the core of most wildland recreational pursuits. Regulations are particularly undesirable toward the primitive end … where regimentation is supposed to be low."[36]

---

[33] For example, Welton (1987).

[34] Quoted in Wall and Wallis (1982), 345.

Even when they face compelling ecological evidence, backcountry users can still resist regulations. Jalkotzy, Ross, and Nasserden noted that "[a]ll resource users need to accept the basic tenet that disturbance corridors are detrimental to wildlife.… [However, this] support is often difficult to obtain from recreational users who feel they have a right to use new access roads developed on public land."[37] For this reason, Fuller noted that "[p]owerful opposition can … be confidently predicted to attempts to limit freedom of access to, or activity within, [Canadian] National Parks."[38] In the end, opposition to regulations and restrictions can arise even in the face of mounting evidence for the damage and impact wilderness use is having on those areas.

---

[35] Shirer (1985), 88. This idea of having no obligations is not that dissimilar from irresponsibility. The pairing of freedom from obligation with irresponsibility is one reason many researchers put forth for increasing the regulations in the backcountry.

[36] Hammitt and Cole (1987), 209.

[37] Jalkotzy, Ross, and Nasserden (1997), viii. See also Davenport et al. (2002), who studied public reactions to management plans that would place restrictions on users in Yellowstone National Park. One of the four types of response was concern over "how their experience would be restricted in terms of access, time, and freedom. While these visitors weren't necessarily against wildlife preservation, they were hesitant when preservation means restricting their own experience," 60.

The freedom promoted by OR is often the freedom to be an individual with few behavioural restrictions. As McAvoy stated, in earlier times wilderness users were "imbued with the pioneer spirit of … hunting, fishing, hiking, mountaineering, and canoeing. They minimized the role of rules and preconceptions, sought independence from social constraints, and were willing to take the forces of nature at face value without the protection of social institutions."[39] Things have changed since then, as McAvoy suggested. Today we see "a wildland-oriented population desiring a more social environment in the woods. Many of today's wildland users desire … to be introduced to the wilderness in a group situation within a structured, controlled, role-defined framework where there is a teacher/protector/manager."[40] Even here, however, one of the guide's roles is to facilitate groups doing what the individuals would not be able to on their own. In this sense, guided wilderness experiences are also about extending people's ability to be in wild nature and go where they want. Furthermore, while there may be more than one person in a group, research suggests that most groups have a low tolerance for meeting other groups.[41] It may be a more social environment now, but we should not confuse a more social atmosphere with a necessary decrease in the emphasis on freedom and autonomy.

---

[38] Fuller (1975), 12.

[39] McAvoy (1987), 459.

[40] McAvoy (1987), 459.

[41] See Laven, Manning, and Krymkowski (2005) and Newman et al. (2005).

In almost all cases, OR research supports some regulations in an effort to protect the integrity of both the environment and the wilderness experience. User limits are one form of restriction that has become more and more accepted as the numbers of backcountry visitors increased. These limits do restrict people: "[c]onsider the places where managers have dealt with heavy recreation demand by limiting the use of popular trails and destinations. One result is that spontaneity and freedom of access are reduced."[42] The real question for these researchers is not whether there should be limits and restrictions, but what kind they should be. In accordance with the emphasis on freedom, regulations that are the least obtrusive are the most desired. As Lime and Stankey noted, managers can influence people's behaviour in more subtle ways, ensuring compliance without interfering "directly with the visitor's freedom of choice."[43] One of the ways they suggest for the manager to require "recreationists to demonstrate a certain minimum level of knowledge or skill before they are eligible to participate in an activity or enter an area."[44] This idea has been around since at least 1940 when Wager proposed it[45] and is one example of Foucault's rules of exclusion: the privileged or exclusive right to speak.[46] Those who have particular knowledge and skills are encouraged to speak in certain ways while those without that knowledge have a harder time. This practice legitimates a subject position that can be more fully occupied the more an individual displays the proper knowledge. Once an individual occupies this privileged position, he or she can speak and act with more authority and power in greater circles of influence. Thus, guides and others who have passed such entrance requirements become spokespeople for that wilderness area and bear some of the responsibility for the behaviour of people in their charge.

---

[42] Cole (2000), 7. See also Marsh (1978) who commented that "[w]hile the regulations generally are introduced to protect the environment and the visitors undertaking a recreation experience, they do restrict freedom, produce a *clientele shift*, and ultimately detract from the wilderness recreation experience in the national parks," 167. Hart (1998) also noted that "[i]t is hard to be comfortable with … restrictions. One of the chief pleasures of the land beyond the roadhead has always been the independence of the traveler there, the freedom from formal rules," 5-6.

[43] Lime and Stankey (1979), 113.

[44] Lime and Stankey (1979), 115. This point refers to individuals or private group trips more than to the packaged tours that have recently become more popular. Tours offer the possibility for those without the required skill level to access these places; however, many regulations still apply. User limits are one example of a regulation that applies equally to tour groups. In addition, there are many restrictions on clients' behaviour on guided tours that do not apply on private trips. In a sense, the responsibility for ensuring the proper skill level for entry into an area is shifted from the individual and placed on the guides and the company. It is not a question of whether there are restrictions or regulations; it is a question of what kinds of constraints there should be and who should bear the responsibility for them.

[45] See Knopf and Schreyer (1985) for a discussion of this type of restriction. Miles (1985), too, suggested that "[a] seminar on minimum impact should be a regular feature of outdoor activity," 99. Presumably, those that do not attend such a seminar or fail to complete it would/should be barred from entering areas of extreme fragility. Cole (1995c) also argues that this is a commonly mentioned indirect technique.

The suggestion that certain users might be restricted from fragile areas has not been enforced in any systematic way in Canada; however, the very idea embodies a central dynamic in the OR literature. The tension between freedom and regulation and the practices of division, classification, and rejection are evident in the notion of entrance requirements. At the same time as Lime and Stankey were advocating for entrance requirements, they were careful to note that, once inside, people were "essentially free to do as [they] please, consistent with certain rules of safety."[47] Those regulations or restrictions that protected wilderness and at the same time could be argued to have little or no effect on people's freedom received the most support.

Underlying the idea of minimum impact seminars are the techniques of division and classification. Although never put into operation, the schematic of power entailed in the suggestion could take many forms (from simple participation to pass/fail and grading classifications). Regardless of how these hypothetical seminars or entrance requirements might be operationalized, the very suggestion that potential users participate in seminars creates the possibility for distinctions that otherwise would remain absent. The seminars inaugurate new types of divisions and classifications that are meaningless without them. The potential to distinguish those who have taken (and passed) a seminar from those who have not is introduced by the suggestion of entrance requirements. The ability to distinguish one person from another is part of how modern power operates: the more finely individuated someone is, the more power has been exercised to mark and inscribe him or her. Once the subject is known, treatment follows suit. If the seminars were mandatory, for example, those who have taken it would be treated differently than those who had not.

---

[46] Foucault (1972b), 216. See also Foucault's discussion of the various enunciative modes in Foucault (1972a), 50-51.
[47] Lime and Stankey (1979), 115.

This issue of entry requirements has appeared outside academic circles, as well. The US Forestry Service offers an example of a more practical approach to entry requirements. The Forestry Service published a booklet on the low-impact program Leave No Trace! (LNT) in which readers were told that they "must answer 70 percent of the following questions correctly in order to pass this test and become certified in Wilderness Skills. Are you ready? Is your pencil sharp? When your instructor signals, start the test."[48] Without passing this test, people could not be certified in Wilderness Skills.[49] Instead of absolute limits, an examination was adopted as a normalizing and standardizing disciplinary technology. The tactics are subtle, as are the requirements of minimum impact itself: the technique of examination attempts to embed compliance to regulations into all backcountry users so that, when they enter, they take the regulations and prohibitions with them. The LNT program's examination makes it more likely that the graduates are the ones who have incorporated these principles and actions into their lives. Since the LNT program forms the standard to which most wilderness agencies and OR companies in the US and Canada adhere, the examination constitutes a powerful and far-reaching disciplinary method. The LNT exam is an interesting example of disciplinary technologies in that there are no formal consequences around access to wilderness as a result of failing the exam. What the brochure (and the LNT program in general) did was to make more visible the markings that distinguish a competent outdoor user from one who lacked the requisite knowledge and skills. The very idea of a test contains within it a classificatory element. People going into the test situation cannot be divided according to their performance in the test. It is only afterward that differentiating users in terms of a standardized set of knowledge and skills becomes possible.

---

[48] United States Department of Agriculture Forest Service (1994), 21.

[49] It should be noted, however, that this certification is not required for entry into wilderness areas in Canada (or the USA).

Over the years that the LNT program has been promoted, OR researchers found that compliance with the standards were not guaranteed simply by success on the LNT test. In other words, people may know what they should or should not do in the wilderness, but not all complied with these strictures.[50] In all exercises of power, opportunities for resistance are opened. Thus, users who behave properly for their LNT instructors and get certified in Wilderness Skills may choose not to behave similarly once out of sight of their instructors. This possibility is increased when one remembers that a common aspect of the wilderness experience that many users seek is freedom from external constraints. This desire runs counter to compliance with LNT standards. However, all exercises of resistance create their own possibilities for the further deployment of techniques of power/knowledge.

---

[50] For examples of this see Christensen and Cole (2000), Confer et al. (2000), Daniels and Marion (2004), Hammitt and Cole (1987), Jones, Hollenhorst, and Tino (2003), McAvoy (1985 and 1987), McCool and Cole (2000), Miles (1985), and Newman et al. (2003).

The idea that users could abandon the LNT practices once out in the wilderness opens the possibility for an increase in surveillance. As the general knowledge of the LNT program spread, the possibility grew of other users witnessing infractions of the minimum impact strictures. Negative judgements of fellow users' level of skill, knowledge, or compliance now had a common yardstick, and it was possible to feel that others would view one's actions as inappropriate compared to the norms of the discipline. On an Outward Bound (OB) trip, for example, one student at the back of the hiking line "stopped and picked up an orange peel that one of his group members had dropped. He turned … and somewhat angrily said, 'what do they think they are doing?'"[51] This student was the only one in the group who had previously attended an OB course. The instructor commented that "the power of that moment, for me, was that this young man had internalized an environmental ethic and a sense of responsibility for the natural world…. He intuitively appreciated the value of the natural world and provided his peers with a role model of earth stewardship."[52] This ethic is labelled as natural and intuitive; furthermore, OB participants are thought to invariably adopt such a position. Whether or not we agree that such an adoption is natural, intuitive, or inevitable, the disciplinary effect cannot be denied. Fellow hikers would feel the sting of such comments, especially when sanctioned by the trip leader. The same dynamics apply to private group use as well. Meeting other groups of users now brings about the potential for your actions to be judged. How you pitch your tent, where you pitch it, what you do with your food waste, and many other aspects of camp life become moments scripted with ideas about acceptable and unacceptable behaviour. While many of these assessments would lack the backing of a formalized authority structure (such as a trip leader), their scope of application was greatly increased. The possibility of encountering another hiker around the next bend, another canoeist on the same lake, or another group at your campsite could never be completely discounted; therefore, potential judgements of your behaviour became ubiquitous.

---

[51] Abbott (1995), 23.
[52] Abbott (1995), 23.

Once rules were set for the size of groups or the number of users per day, a whole host of formal procedures and markers arose. In some cases, where user limits were applied, it was possible for the authorities to monitor the number of people signing in at the trailhead. The West Coast Trail on Vancouver Island is a good example of this. There are two access points for the trail (at the north and south ends) at which users are required to register when they begin. Once the total number of users for the day has entered the trail, no more are allowed on the trail until the next day. Also, as of the early 1990s, permits became mandatory. This registration system is similar in many popular backcountry destinations in North America. Registration tags also were instituted; these were to be displayed on one's tent or backpack to distinguish legitimate from illegitimate users. In Jasper and Banff national parks, backcountry users must register and pay for the number of nights they plan on being in the backcountry. The tag they receive is to be displayed on their tents while they are in the backcountry. Designated campsites arose in many protected areas in Canada with specified numbers of tent pads, and prices were based not only on the number of nights spent in the backcountry, but also on the number of tents.

These disciplinary mechanisms elevated surveillance ever nearer to a pure, unrestricted flowing gaze exercised from anywhere or everywhere and thus nowhere.

Even at the ultimate functionality of surveillance, however, discipline remains incomplete. To be a disciplined subject requires compliance. Backcountry users might have a heightened sense of the potential for surveillance, but, unless they internalized the normative standards urged upon them, their compliance would not follow except from accidental coincidences or forced compliance (as when hikers are turned away after user limits are reached). Receptivity occurs because the subject has been made more compliant, less resistant, or more docile. Increased docility can be a measure of the internalization of disciplinary techniques and norms.

Many LNT principles were specifically designed to encourage and foster this internalized monitoring of behaviour because it was more efficient for backcountry users to adopt the guidelines on their own than to have to enforce the rules through external means like wardens. As in the examination that has no formal jurisdiction, so in the LNT program many of the methods employed are subtle. For instance, the section in the Forest Service's LNT booklet on staying on the trail and not cutting through switchbacks is framed from a subtle internal and subjective viewpoint. The illustration shows two hikers, one with a happy smile, looking up at a series of switchbacks. The text accompanying this cartoon reads: "[s]tay on the trail! You will feel better, and the land will look better."[53] Not cutting switchbacks, the reader is told, is easier and it saves the land from ugly gullies that wash away valuable topsoil.[54] The more successful these discursive techniques are, the more deeply embedded they become in those who adopt them. It is an issue of compliance, but compliance with the least resistance. The possibility of resistance exists, certainly, but it becomes less and less likely the more deeply adopted these techniques become.[55] The whole 'discursive push' makes cutting switchbacks less appealing. At the same time, heavy-handed enforcement mechanisms are rendered superfluous because disciplinary power operates internally to encourage compliance. I am not suggesting that there are no police in wilderness; in true panoptic fashion, users become the police as well as those subjected to the policing.[56] As the Forest Service states, when leaving, backcountry travellers must "police the area to make sure you **Leave No Trace!** of your visit."[57] If users can be persuaded to comply without being forced, they will retain feelings of freedom.[58] By claiming compliance is easier on hikers and by associating deviation with guilt over 'ugly gullies,' LNT encourages compliance in the sense that users who associate guilt with infractions of LNT strictures are more likely to obey. In other words, they become the agents disciplining themselves.

---

[53] United States Department of Agriculture Forest Service (1994), 9.

[54] Miles (1985) suggested that climbers who wished to "shave fifteen minutes off their descent time by short-cutting a trail may contribute to a scar in the side of the mountain that could take many years to heal," 98. Again, behaviour that does not follow the minimum impact standards is phrased in terms of serious consequences. The opposition of fifteen minutes of time-saving to years of healing makes clear what is at stake.

There is an interesting relationship between LNT and the more overt, external constraints such as regulations and restrictions. Hart recognized that external regulations

---

[55] To be clear, I am not making a value judgement about the goal of these techniques. My comments are aimed at understanding the rules surrounding the use and effects of these techniques, not whether they are good or bad. As it happens, I am in favour of many techniques designed to preserve certain types of landscapes. However, I remain unconvinced that the field of OR understands their functioning very well in terms of the rules of production and discursive practices that underlie them.

[56] The Panopticon was a schematic for a new type of prison that would allow guards in a central tower to keep watch on inmates. The unique element in the Panopticon was the invisibility of observation: guards could see prisoners, but prisoners could not see the guards. Over time, the prisoners would begin to internalize this invisible observation to the degree that eventually guards would be superfluous because inmates would have begun to police themselves. See Foucault (1995) for a detailed discussion of the Panopticon. The extent to which this policing can be taken is quite fine. Hart (1998), for instance, reminds us that "[s]leeping spots need policing too," 273.

[57] United States Department of Agriculture Forest Service (1994), 12.

[58] See also Daniels and Marion (2004) who suggest "that the LNT program should focus more on our fundamental belief system, including morals that help us define what we understand to be good and right behavior.... The importance for this is supported by McGuire's model of persuasion.... McGuire's model has six components, each essential for persuasion: exposure, attention, comprehension, yielding, retention, and actions (behavior)," 16. That yielding is essential in this model illustrates again the docility necessary for LNT. Attarian (1996) also suggested that we place more emphasis on the affective domain because, if we can change values, "then changes in behavior will result," 41.

(for example, user permits) would not suffice to safeguard our wilderness. Instead of relying solely on these forms of constraints, he advocated, every backcountry user must "develop a personal, knowledgeable code, subtler and perhaps stricter than any set of standardized rules can possibly be."[59] Here Hart opposes the internal to the external, or regulations to freely chosen personal codes. This personal set of rules is, in actuality, developed by other agencies (like the Forest Service or Parks Canada) and standardized into programs (like LNT). These programs become more effective the more that users believe they have adopted this code on their own. The LNT program contains the same external standardized rules Hart claims are not as effective; yet, it is these rules that become internalized as personal codes. How can Hart claim that external standardized rules are insufficient and then propose another set of standardized and external rules as an alternative? This is, I argue, one of the defining elements in disciplinary power; it makes people feel as if they came up with the idea and therefore can retain a sense of individual autonomy and freedom—traits many see as essential backcountry use. Hart continues by suggesting that users should watch not only themselves, but also others. By keeping an eye on each other and bringing the manager's attention to issues and concerns, "[w]e can praise and blame, suggest, kibitz, and have a real influence on what is done."[60] Thus, the personally developed, internal code of ethics gets enforced on others. The distinction that this is an internal code not an externally imposed one is hard to see.

Another example of the subtlety of persuasion comes from a manual for teaching backcountry travel: *The Backcountry Classroom: Lesson Plans for Teaching in the*

---

[59] Hart (1998), 6.

[60] Hart (1998), 6. Another tactic used to discipline users flows from Hart's comments. If we should blame and kibitz those who do not adopt minimum impact techniques then these users become irresponsible. Miles (1985) suggested these kind of users were just "unwilling to behave themselves properly," 96. Non-compliance is now linked with defiance, unwillingness, unruly attitudes, and irresponsibility.

Wilderness. This manual suggested that facilitators responsible for teaching backcountry skills should guide discussion by helping students focus on any evidence of other humans (for example, litter or trampled plants). Then the questions should turn to evaluative or speculative ones: "Do the actions of these people tell us anything about their values?"[61] The answer provided is "[y]es. Their behavior reveals their values.... These actions tell us that they do not value the wilderness or the wilderness experience as much as those who do practice minimum-impact techniques."[62] On the other hand, for people who practice these techniques, "it is safe to assume that they value both [wilderness and the wilderness experience] highly. It is their concern for wilderness that has caused them to consciously restrict their own freedom of action in the backcountry for the sake of preserving it for themselves and others."[63] This explanation does not take into account the array of mechanisms and systems designed to enhance compliance and docility; therefore, to say people 'freely chose' to restrict themselves is not entirely accurate.

Information is another subtle disciplinary method used in OR.[64] Information can cause people to change plans and/or behaviour while retaining a sense that they initiated the changes. That is, when users telephone to get information about trail conditions and regulations/requirements related to a specific region of backcountry, they may decide that the trail is not for them after all. In cases of heavy-use conditions, some users would change plans and go elsewhere or return to the same place at a less busy time. Emphasizing these conditions and providing users with this information, OR scholars have suggested, is one unobtrusive method for restricting the number of users. Even information given out at the trail office or trail-head might prompt people to reconsider their destination while at the same time thinking they made the choice themselves. Staff at national parks who provide such information to users could be influential in the process of determining which users go where. The power of information as a disciplinary mechanism lies in its ability to produce conformity to the rules without overtly appearing to do so, not in its elimination of all constraints. In other words, it appears to maximize both constraints and freedom. Gunderson and McAvoy claimed that information was "the most effective light-handed management strategy to reduce impacts and conflict in wilderness, while retaining visitors' freedom of choice."[65] There are, however, still risks associated with information: "[w]hile providing information is promising, managers must avoid providing too much information and taking away the sense of discovery and exploration that is important to many recreationists."[66] The balance between freedom and regulation shifts. Too much information or too many regulations can tip the scale away from the values associated with freedom: a sense of discovery, the ability to be oneself and go and do what one wants, and so forth. Information is promising because it is light - handed and thus can be used more often seemingly without weighing down the scale.

---

[61] Drury and Bonney (1992), 42.
[62] Drury and Bonney (1992), 42.
[63] Drury and Bonney (1992), 42-43.
[64] See, for example, Cole (1997), Cole, Hammond, and McCool (1997), Duncan and Martin (2002), and McCool and Cole (2000) for studies on the effect of signage on compliance with minimum impact standards.

The issue of freedom and regulations is a prominent concern for OR. The debate has centred on determining the appropriate level of regulations needed to ensure a certain level of protection and use. The desire to maximize the various types of freedom frequently ascribed to wilderness use is common in this debate; however, the idea of complete freedom is paradoxical. Rules are necessary; thus, while we may take issue with some of the rules and strictures of minimum impact, we cannot abandon the idea of rules altogether. Instead of arguing for the abandonment of all the rules for backcountry travel or simply arguing for changing the rules, I am arguing for a change in the rules of formation that produce the LNT rules in the first place.

---

[65] Gunderson and McAvoy (2003), 38.
[66] Hammitt and Cole (1987), 260.

When Foucault discussed the panoptic structure of education and the classroom he stressed the presence of the teacher who could, because of factors ranging from seating plans to behavioural dictates to regimentation to spatial segregation, continually surveil the students. The same holds for prisons where cameras, strict timetables, segregation, and regimentation all increased the power of surveillance. In the case of minimum impact education, the guard/teacher is placed inside the wilderness traveller: "[w]e will aggressively impress upon wilderness users the 'Leave-No-Trace' ethic … and other types of zero impact awareness."[67] Now, there is no escaping the potential panoptic gaze because it travels with you: we might begin "to have the feeling of someone looking over your shoulder, in thought at least, when you stop for the night."[68] Linking the policies of LNT to wilderness areas means that the farther into the pristine backcountry one goes to escape the constraints of society, the more the LNT principles get activated: policing ourselves becomes ever more acute the farther we go into wild and 'free' areas. Disciplined subjects are enabled in this sense. Compliance assures admittance and continued use. This disciplining, a form of auto-surveillance perhaps, would reach total efficiency at the point where all are subjected to the regulations, all adopt them, and all feel they chose to do so.

---

[67] Kennedy (1996), 8. Miles (1990) has also recommended that we "should program a proper wilderness-use ethic into the experiences of … students," 327.
[68] Hart (1998), 228.

Whether through LNT programs, information signage, pamphlets, stickers, slogans, educational classroom settings, or examinations and certification, indirect methods of regulating the impact of backcountry use are preferred. McAvoy summarized it nicely: "controls should be the minimum level needed to solve the problem."[69] Indirect methods are preferred by many managers because visitors are less likely to feel controlled. The less visible the control, the more freely power can be applied to its object. That is, power flows smoothly and without interruption towards its object. Disciplinary techniques are not, however, omnipotent, nor will they always achieve their end; they are but one important element in the operation of power. The more that the exercise of power creates resistance, the less effective it will be in disciplining. Another element is the amplitude of the power. The more forcefully it is applied, the more effective it can be. The pure application of disciplinary power would maximize both its invisibility and its amplitude. The third element in the deployment of disciplinary power is narrowness of focus. That is, the more focussed its application, the more effective it will be. These facets are all prominent in OR literature as it seeks the pure movement of power in order to eliminate particular impacts of backcountry use.

Even though, as Marion and Leung claimed, through education and regulations "managers can influence or control all use-related factors," the disciplining function of minimum impact is not complete.[70] LNT principles, for instance, were not designed to account for every possible situation; some individual assessment and judgement will always be required. In areas in Canada where land uses such as wildlife preservation take priority (for example, in biological reserves that protect an ecosystem), leaving the decision to the individual user may not be appropriate. In cases of areas of fragility and/or rarity, another element of the disciplinary structure is often brought into play. Since the popularity of minimum impact programs began growing in the 1970s, OR has advocated different strategies to address the problem of protection by means of regulation. An early strategy was suggested by Marsh in 1970. He posited that if users were not qualified then "access may only be permitted to organized groups with an official guide."[71] Marsh's strategy authorized the official guide (not just a guide, but an *official* one) to speak about, interpret, and protect the area. This constraint proclaimed that there was a correct way to be in a particular wild place, and only the guide knew it. In this way, techniques of normalization were brought to bear on minimum impact standards. The guides were trained according to standardized criteria that were then presented as *the* way to be in the wilderness. Guides were trained according to the same minimum impact standards across Canada. As long as guides followed their training, they could refer sceptics to the literature on minimum impact to prove they faithfully adhered to the standards. Furthermore, proponents of minimum impact could rest assured that, due to its prescriptive and repetitive nature, their message would not be distorted easily. This internal organisational schema is largely responsible for the uniformity of minimum impact principles throughout North America and contributes to the disciplining functions of the discourse on minimum impact.

---

[69] McAvoy (1985), 18. In some cases, however, the minimum required to solve the problem is quite high. Managers in the Grand Tetons did "not believe that they could reach enough climbers with educational efforts to make a significant change in behavior and reduce the impact problem. They favored rehabilitation and user control" (Miles 1985, 99).

[70] Marion and Leung (2001), 21.

Another method for normalizing minimum impact standards is to make them an issue of 'common sense.' According to this method, adopting minimum-impat standards "would really only introduce common sense into contemporary American camping.

---

[71] Marsh (1970), 132.

Minimum impact camping is as essential to wilderness as safe driving habits are to the highway."[72] Not only is common sense invoked here, but also the issue is linked to another highly charged topic: safe driving habits. It would be criminal to argue against good driving habits and effective highway legislation. The link between safe driving and compliance with minimum impact standards makes it seem criminal to argue against either. These techniques of normalization suggest that wilderness is a space in which only certain people or behaviours should be tolerated. It is a place that required work to access, work in both the sense of physical exertion and the sense of training and qualification. This attitude is reflected in Miles' comment that "[w]ithin one campsite, not far from the edge of a lake, we found human waste and toilet paper simply thrown on the ground—and this was a tough seven miles from trailhead where only the true wilderness buff should be!"[73] One needed to be true, trained, and technically competent to be in the wilderness. Those that were not were portrayed in the discourse as irresponsible, lazy, or uncaring.

In conclusion, support for regulations and restrictions is usually couched in two forms: first, regulations protect the wilderness ecosystem, and, second, they protect the type of experience that users want. In terms of the first form, the irony is that ecological knowledge and theory appear infrequently or relatively superficially in OR discourse. Even when arguments are made about the ecological value of minimum impact practices, very few details accompany them. As I suggest in Chapter Four, the lack of detail results from a shift from nature itself back onto human users and their experiences. Furthermore, I contend that this shifting is not necessarily a product of a misunderstanding by OR researchers of what they were arguing for or against. Instead, I suggest that another reason may help explain why the discourse in OR on freedom and regulation has a difficult time staying focused on the ecological dimensions: the very notion that people are separate from wilderness is paradoxical and cannot be maintained cleanly. Thus, the problem of slippage cannot be addressed directly without our taking the deeper paradox into account.

---

[72] Bachert (1990), 87.
[73] Miles (1985), 97.

This deeper division between nature and society is a key aspect in minimum impact policies, statements, programs, and studies. The idea that people must remove all trace of themselves in the wilderness is enabled by the view that there is something inherently different between people and wilderness, and that people, therefore, damage it. Furthermore, this division of humans from nature is a condition of thought rather than an object of thought in most OR discourse. That is, discussions of regulations and freedom begin with the premise of a specific kind of difference between people and wilderness. Starting from the position that humans are distinct from and damaging to nature makes it harder to ask whether human impacts could be anything other than negative. Even in discussions that specifically target ecological impacts (for example, trampling studies), the question of whether or not any of these impacts could be positive or at least neutral rarely arise.[74] The idea that all human impacts are negative and therefore must be eliminated as far as possible (through regulations, restrictions, and prohibitions) forms the condition upon which OR builds its recommendations and struggles to find solutions; it is not often an object of thought deserving of evaluation in itself.

Change and Authenticity

---

[74] For example, see Daniels and Marion (2004), Gibson et al. (2000), and Monz (2002).

Things alter for the worse spontaneously, if they be not altered for the better designedly. ~ Francis Bacon

Change is inevitable—except from a vending machine. ~ Robert C. Gallagher

In addition to the debate over freedom and regulation, another central topic in OR is the problem of change and authenticity. This problem has proven to be another paradox. There is an incompatibility between the idea of authenticity and that of change. Can something authentic change? Copies that are faithful to the original are thought to be authentic, but copies are never identical to the original, so change must be acceptable in some forms or amounts. The original is used as the benchmark against which to determine the authenticity of future iterations. Authentic nature is usually also original. Authentic nature can manifest in many ways (for example, the Garden of Eden was original but different from the idea that nature is the original storehouse of resources or that nature is the original base from which culture has grown).[75] In 1995, William Cronon also proposed that wilderness has been seen differently at different points in USAmerican history.[76] Advocates for Cronon's position were proposing that nature in general and wilderness in particular are products of culture as much as anything else. One of the implications of the idea that wilderness is a social construct is that it is also a located concept. That is, wilderness is cultural, and understandings of what it is and what constitutes its value vary across cultures and time. The difficulty with authenticity and change begins with this recognition. If wilderness is often seen as an original place that existed before human culture arose, then it cannot very well be a cultural product. When nature is seen as an authentic original place, only changes that are natural belong; cultural and social factors disturb wilderness.

---

[75] See Altmeyer (1995) who suggested three ways in which nature was viewed in Victorian Canada.

[76] The 1995 volume edited by William Cronon was one of the most influential books to make the claim that wilderness is a product of cultural forces. This book, *Uncommon Ground: Rethinking the Human Place in Nature*, spawned another edited volume (*Reinventing Nature?: Responses to Postmodern Deconstruction*) that argued against the claim that wilderness was socially constructed. The next year, *Environmental History* sponsored a lively debate that began with Cronon's article and then several responses to it, some which were quite strongly opposed to Cronon's position. Although there are proponents of both views, *Uncommon Ground* introduced into the discussion concepts that have since become ubiquitous. Whether or not one agrees with the idea of social construction, the debate is framed around this idea much more now than ever before in the past. See Cohen (1996), Cronon (1995a and 1996), Dunlap (1996), Hays (1996), and Soulé and Lease (1995).

In North America, the concept of wilderness has many common features that are present in certain amounts and with certain relations that form the specific discursive assemblage we call wilderness. The centrepiece of this discursive structure is 'naturalness,' which most often means that ecological processes operate independently of human influence or control. The idea of naturalness is a complex and problematic one that in OR is subjected to a number of rules of formation. I focus on the non-religious rules for formation because, as scientists, ecologists rarely use religious language to examine nature. Religious formations can be left aside as they are a minor influence in the linking of ecological and OR discourse. The dispersal, then, of the tributaries of the discursive object 'naturalness' (for example, pristine, unspoilt, wild, original, authentic, nonhuman) places them on a shared surface. In other words, the discursive assemblage of wilderness includes objects that are produced and arranged according to similar rules. As an example, the rules of formation in OR produce 'wild animal' as distinct from 'domesticated animal,' even when they are of the same species. Then there are the relations between the various objects in OR. These relationships are also subjected to a discursive practice that centres on the notion of naturalness. So, for instance, wild animals are set in relationships like predator-prey dynamics, which is a different type of relationship than the one between trained domesticated animals. The OR discourse on wilderness throughout in Canada and the USA operates in a fairly consistent manner to produce a narrow range of objects. The emphasis in this discourse has been on natural objects in natural relationships. Other types of discursive statements are certainly produced (for example, there is a discourse on constraints and barriers to participation in wilderness activities), but these have minimal connection with ecological discourse.

The emphasis on authentic naturalness as an organizing principle in OR supports the conclusion that natural nature is the original state of affairs, before the coming of humanity, and any alteration to it has the potential to ruin it. Furthermore, this state of affairs must continue to manifest authentically or naturally. Foucault's notion of the surface of emergence from which discursive objects are formed helps to frame this process.[77] Authenticity and naturalness form the surface that gives rise to many of the various and conflicting objects in OR. For example, conflicting values and behaviours associated with motorized versus non-motorized outdoor recreation reveal themselves against the surface of authenticity, that is, what nature truly is or should be. Hikers argue against mountain bikes in part because bikes are thought to destroy nature more than feet. Jet skis have no place in many protected areas in Canada because they are seen as antithetical to natural nature. While the meanings change and objects come and go in the discourse on wilderness, the centrepiece, the surface of emergence has remained stable over most of the twentieth century.

Just to the east of Edmonton, Alberta lies Strathcona County. The county's parks and recreation department provides an illustration of the application of rules of formation in the production of its outdoor master plan. In this document, the county explained the preservation of natural resources as the "retention of the physical and biological integrity, authenticity and intrinsic value of a resource in perpetuity."[78] The ideas of naturalness and authenticity need to remain in perpetuity or the wilderness quality of the area becomes threatened. The visitors may change, what they do while in Strathcona County may change, even the land itself can change (as long as the changes occur as a consequence of nonhuman impact), but the authentic naturalness of the place cannot change without a loss occurring.[79]

---

[77] Foucault (1972a), 41.

The difficulty with the concept of wilderness in OR lies in trying to ascertain exactly what constitutes the original, authentic, and natural condition and then how to keep it that way. Moreover, just how long is 'in perpetuity'? On the one hand, original wilderness requires the land to remain much as it was in the past and to remain unaltered into the future, and yet obviously there have been changes to the land over the course of time. At some level, the idea of constancy cannot be reconciled with that of change. As a result, a problem arose in outdoor recreation: which changes are acceptable and which threaten the core components of wilderness? This question has been addressed by OR scholars as they have grappled with ways of protecting valuable processes, landscapes, species, and also the types of experiences one can have in those areas.

The problem of authenticity has led to numerous attempts to explain the differences in the various types of changes. Most commonly, the attempt to divide and reject types of change arises within the broader, complex understanding of natural versus unnatural. It is this understanding that initiates the arguments over the composition of wilderness areas, their future, their value, and their use. The Recreation Opportunity

---

[78] Strathcona County Parks and Recreation (1987), 6.

[79] A similar temporal dimension can be seen with respect to the US Forest Service. For the Forest Service, authentic wilderness meant that "[t]he scenic beauty of the land is much as it was in centuries past" (United States Department of Agriculture Forest Service 1994, 2).

Spectrum (ROS) is one such attempt. At the local, regional, or national level, the ROS attempts to provide a balanced spectrum of opportunities that are geographically equally distributed. For instance, non-motorized travel is allowed in most classes of wilderness, whereas motorized travel is restricted to more developed end of the ROS. In addition, the types of services and amenities are determined by the ROS: flush toilets at the developed end, few or no services at the wilderness end. Another model, Limits of Acceptable Change (LAC), is closely connected with social carrying capacity. Social carrying capacity refers to the expectations visitors have of how many others they should meet and the amount of noticeable impacts they should encounter. Once the carrying capacity is determined, the level of acceptable change will be set accordingly. In other words, the LAC model argued that more developed areas can tolerate a higher limit of acceptable change than primitive wilderness areas.[80]

Quite often, the two concepts, natural and unnatural, are mutually dependent. That is, those changes that do not destroy the 'authenticness' of wilderness are, perhaps obviously, the natural ones, and, thus, all other changes are unnatural. However simple this division might appear, it has proven quite difficult to accomplish it cleanly. As a result, it has been nearly impossible for OR to reject one side of the binary without placing the other side in jeopardy.

For instance, Hammitt and Cole summarized their seminal book on the ecological impacts of recreation with the following.

---

[80] See Boyd and Butler (1996), Cole (1995b and 1995c), Driver et al. (1987), Hammitt and Cole (1987), Harshaw and Tindall (2005), Jacobs and Schloeder (1992), Kreutzwiser (1989), Laven, Manning, and Krymkowski (2005), Lindberg and McCool (1998), Manning (1999), Roggenbuck, Williams, and Watson (1993), Shafer and Hammitt (1995), Wall (1989) and Watson and Williams (1995) for more details on both the ROS and LAC models.

> Change is an all-pervasive characteristic of natural environments. The norm in undisturbed wildlands is continuous change—*succession*—to use the ecological terminology. When people are introduced into the natural scene, particularly when they come in large numbers, the natural direction and rate of change are often altered. In many cases ecosystem processes are accelerated.[81]

The problematic nature of change is highlighted here. Change is natural, but unnatural change is not. Distinguishing more clearly than this would seem at first to be an easy task. Indeed, Hammitt and Cole had already explained earlier in their book that "[c]hange in nature is the norm; the natural variation in the rate and character of change is acceptable…. Changes beyond this constitute man-caused change or impact."[82] So when changes occur that are beyond the rate or scope of those that occur naturally, we can state they are unnatural and caused by humans. For example, as rivers straighten over time, strainers (trees and logs that extend out from the banks over the river and are highly dangerous to boaters) are removed naturally by the current. This process of straightening was occurring before the purposeful removal of strainers for recreation safety began. Thus, the removal of strainers is "supported as long as it is done in a manner that approximates natural strainer removal."[83] In Hammit and Cole's summary, humans are defined as separate from nature and everything we do damages nature. In the second quotation, only human activity that does not mask itself as natural is damaging. The argument breaks down thus: humans are not natural because they cause unnatural change, that is, change beyond the scope of what occurs without human influence. In addition to the circularity of this argument, we have the problem of distinguishing between changes brought about by nature (that is, natural changes) and those that appear the same but are made by humans.

---

[81] Hammitt and Cole (1987), 195-196. See also Daniels and Marion (2004) who similarly define change by noting that "[r]ecreation visitation to protected environments can lead to unacceptable levels of [impact, which,] when severe … compromise management goals by degrading natural conditions and processes … and the quality of recreation experiences," 2.
[82] Hammitt and Cole (1987), 20.
[83] Campbell (2002/2003), 53.

Nearly ten years after Hammitt and Cole's book appeared in 1987, Cole was still grappling with the question of change in wilderness.

> Definitions of naturalness vary but the concept is most often equated with pristineness and defined by conditions that are similar to what would have existed in the absence of post-aboriginal humans.... In this paper I will refer to such conditions as 'pristine,' although this does not imply lack of influence by aboriginal humans or that future conditions should not diverge from the past as the natural processes of geologic, climate, and evolutionary change continue.[84]

Here the problem of dividing the types change and rejecting some arises again. The difficulty begins when Cole asserts that North America was natural before Europeans came, but in the same breath argues that Aboriginal peoples modified their environment, and that the environment would have changed regardless of European presence in North America. The differences between changes wrought by Europeans, those by nature, and those by Aboriginal people are not clear. This passage is emblematic of the trouble paradoxes can cause. The idea of authentic wilderness is central to OR discourse, but at the same time does not permit any clean definition or delineation. Without a way effectively to deal with these paradoxes, researchers were left to wrestle with them as best they could.

Instead of a boundary between Europeans and Aboriginals, Hendee and Dawson use humanity as the boundary marker for acceptable change:

> [w]e define threats to wilderness here as a general concept, focusing on change agents or processes that negatively or adversely impact wilderness resource conditions and values.... We are talking about change agents that

---

[84] Cole (1995b), paragraph 2.

> come directly or indirectly from human influences and not natural disturbances (e.g., lightning-caused fires, volcanoes, hurricanes, etc.).[85]

Rather than split some human change from others, all human changes are seen as negatively affecting the naturalness of wilderness areas. Although this particular form of splitting does avoid the slippery slope toward racism that accompanies attempts to divide some humans from others in terms of which ones are natural and which are not, Hendee and Dawson's definition does not resolve the issue. Take their example of fire. Fire could be an agent of acceptable change, unless practiced by careless humans.[86] That is, lightning-caused fires are fine, but one resulting from a campfire is not. Why are lightning-caused forest fires qualitatively different from ones started by the careless use of campfires? The forest cannot tell the difference, nor can the animals in it. Moreover, in many natural areas fires have been suppressed for over one hundred years. If one burns there now, is it unnatural because the conditions through which it spreads are human-created? The basic question here is 'Is the absence of human-caused fires always natural?' What happens when the decision to have prescribed burns is made? Is it now an unnatural process for park staff to set prescribed fires? If we stop natural fires from threatening human life or towns are we committing an unnatural act? By focussing on the agent of change instead of the change itself, Hendee and Dawson end up trying to distinguish between changes that look the same and have the same effects.

These authors have all assumed that (some) changes wrought by (some) humans are negative ones. The difficulty has been defining what was meant by human or why these changes and not others are negative. The problem lies both in saying that all human change threatens wilderness and that only certain changes do. The problem becomes one of drawing the line around what to include as acceptable changes and then justifying it. Should all changes be allowed? Obviously not. No OR scholar advocates that wilderness can be developed like an urban centre. Some changes are not acceptable in wilderness areas because they imperil the very core of the wilderness. However, this does not necessarily mean that ecological criteria trump all other factors. Hammitt and Cole recognized this in 1987 when they noted that "the decision of where to draw the line is one which can not be determined *entirely* by ecological criteria."[87] Not all change should be stopped and not all allowable change is based on ecological criteria. Wall, probably more so than any other author, acknowledged the difficulty inherent in this task. It does not matter, he says, what impact type researchers are trying to measure; they all face the similar, intractable challenge "of distinguishing between human-induced and natural change, of determining a baseline, of causes separated temporally and spatially from their effects, and addressing the complexity of direct as well as indirect impacts."[88]

---

[85] Hendee and Dawson (2001), 4.

[86] It is interesting to note that it is only recently that OR has been able to see any human-caused fire as beneficial. Prescribed burning in protected areas is still a contentious issue (although for reasons beyond whether fires are natural). Most ecologists and OR researchers now agree that fires are a natural process; however, there is substantially less agreement on what to do with this realisation.

This problem of line-drawing is further complicated by the fact that most authors concede that some human changes are actually good for nature. For instance, in many cases of human impacts the problem is the trampling of plants and the loss of vegetation cover.[89] But these are not always negative impacts: "[u]sually, vegetation cover will … be reduced [alongside trails where trampling occurs], but sometimes the habitat changes will result in an increase in cover there."[90] Wall also cites this sort of change as beneficial to nature: "in some cases habitat diversity may increase as areas are opened up for recreation and light is allowed to penetrate along trail and campsite edges in areas that were formerly entirely forested."[91] Were these changes to occur without any humans present, they could be labelled positive ones, but because hikers caused these changes, calling them natural or appropriate in wilderness areas is much more problematic. If they are positive, then not all human change is negative, even in wilderness areas.[92]

---

[87] Hammitt and Cole (1987), 21.
[88] Wall (1989), 212.
[89] For example, Gibson et al. (2000), Monz (2002), and Pickering and Buckley (2003).
[90] Hammitt and Cole (1987), 68.

Even Hammitt and Cole, having already described as negative all changes that exceed the scope of natural ones, inserted the caveat that "[c]hange is natural; thus management will generally not seek to halt change; rather, it will seek to halt undesirable change.... [In the wilderness] most but not all human-caused change is undesirable."[93] At the end of their book, they changed position yet again: "[w]ildland recreation, because it occurs in natural environments, inevitably causes some degradation of natural conditions."[94] Depending on what section of the book you read, human change can be negative, positive, neutral, natural, or unnatural.

How are we to understand change in wilderness areas, then? It seems that change is most often negative, unless it is natural. That is, positive changes are those that result from natural processes. In other words, changes that are in the best interest of naturally functioning ecosystems, such as an increase in biodiversity, are acceptable. The conclusion that changes such as these are acceptable implies that human-induced change designed to foster natural ecological processes is acceptable. This logic amounts to saying that unnatural change is not good unless it mimics natural, ecological change and then it is positive. However, how can human beings instigate natural change when the definition of natural is 'without human influence or control' ("a place you just let be and let go just by itself," as Glover said[95])? Furthermore, some ecological changes and processes have not always been seen as having a place in the wilderness. Forest fires, overpopulation, and predation are three examples of problematic natural changes/processes in the literature of ecology and OR.[96]

---

[91] Wall (1989), 217.

[92] See Thurston and Reader (2001) who note that both mountain biking and hiking can cause 100 per cent reduction of stem density and species richness, but that none of this degradation was evident after one year. Research such as this indicates that even in cases where human impact is deemed negative, it may not be a lasting change and thus is not as serious a change as was thought. This conclusion raises a question: if the area recovers in a year, would Strathcona County be satisfied that the land was being maintained in perpetuity?

[93] Hammitt and Cole (1987), 196.

[94] Hammitt and Cole (1987), 327.

There are some interesting comparisons between OR and ecology concerning fire, overpopulation, and predation. In the early years of ecology, fire was usually treated as a disturbance or threat. As the century progressed, ecologists began to understand that fire played an important role in many ecological systems. It is difficult to place exact dates on this change as it occurred slowly over fifty years (roughly between 1913 and 1960); however, in OR discourse, fire has been more problematic because of concerns over the impacts of fire on recreation and tourism. Like ecologists today, most OR scholars recognize that fire is a natural force, but they are less supportive when it comes to the question of prescribed burning in wilderness areas. Their reluctance stems not from the view that fire is negative in itself; it comes from the concern over the effects a fire could have on the recreation and tourism potential of the area. Furthermore, safety concerns are a significant factor when it comes to prescribed burns because of the close proximity of towns and other types of development to wilderness areas (for example, Banff and Wasagaming lie inside park boundaries).[97]

---

[95] Glover (2000), 7.

[96] For instance, Cole (1995c and 2000), Cook and Borrie (1995), Frelich and Reich (1995), Glitzenstein and Platt (1995), Marsh (1970), and Vanderlinden and Eyles (2000).

In terms of overpopulation, ecologists are more likely to see explosions of animal populations as a negative or disruptive event, because they are evidence of a system out of balance, likely because of human interference. Attitudes in OR toward overpopulation have changed with time. Today, OR is more likely to recognize the dangers of population explosions in protected areas. In the past, managers have imported exotic species as tourist attractions and promoted the development of certain 'charismatic mega-fauna' at the expense of less 'flashy' species. The last remnants of this practice were removed from Banff only in 1997.[98] In OR, population explosions can be seen as negative or positive (for example, high elk numbers can positively impact tourism in mountain parks).

Understandings of predation have also undergone changes over the years. As an example, for many decades wolves were trapped, hunted, and poisoned across much of North America. Compensation programs were initiated to help ranchers who suffered loss due to wolf kills. The belief that wolves are indiscriminate killers that deserve to be shot has evolved in both ecology and OR into a more tolerant view that sees them as part of wild ecosystems. Nonetheless, an ecologist would not have the same concerns as an OR researcher regarding the value and role of wolves in nature. Ecologists do not have to deal directly with natural threats to humans. In fact, the reverse is more common (that is, threats by humans to nature). In OR, on the other hand, there are real concerns about the dangers wilderness presents to users. In this sense, wolves might be more acceptable from an ecological perspective than they are from an OR perspective.[99]

---

[97] See Page (2000) and Sachroa, Stronga, and Gates (2005).
[98] Page (2000).

The idea that wolves are a natural part of ecological systems but may not be as welcome from a recreation perspective points to one final element in the problem of change in wilderness. Some changes to wilderness are seen as positive not in relation to natural areas or natural processes but in terms of human needs and desires. Wall, for example claimed that "not all changes are bad and the environment is often modified in order that the requirements of human beings may be met more satisfactorily.... Even most wilderness parks are managed: trails are introduced, campsites are designated and constructed, and information and interpretive centres may be built."[100] These changes are viewed as positive because they enhance the wilderness experience and are in addition to ones that increase freedom and choice. Some changes are seen as more than necessary; they are positive and add value to the recreation area. Given the level of human-induced change promoted in this example, it is difficult to maintain a distinction between positive and negative changes along the divide between natural and unnatural. It is also difficult to reconcile this level of development and change with notions of authenticity. It appears that OR wants wilderness areas to remain authentic, that is, in their natural state—even if that state must be built, modified, improved, altered, and changed for its own sake or that of its users.

Individuals in the Woods

I needed to be in the bush. There I find solitude and beauty and purity and focus. That's where my heart lies. ~ Mark Burnett

---

[99] See Foster (1978) and Loo (2006) for an interesting history of wild life in Canada.
[100] Wall (1989), 205-206.

Solitude is independence. ~ Hermann Hesse

As noted above, the structure of OR is similar in many regards to that of science. As products of modernity, both science and OR reflect the will to know. The will to know has been directed toward knowing more about human beings in terms of disciplines such as physiology and psychology. These fields individuate humans beings. They separate people into groups (for instance, there has been a great proliferation of categories of mental illness over the last hundred years). They separate bodies into component parts (for example, biology has divided the body into systems—circulatory, nervous, respiratory—and components—cells, neurons, and tissues). One underlying commonality in these modern projects is the dependence on and further delineation of the individual. This process of individuation makes a person's identification, treatment, education, and punishment (to name a few) both tightly targeted as well as highly dispersed throughout society. That is, training is more effective when it targets an individual's learning style, but it is also more effective when it is more deeply embedded into the fabric of a society.

One consequence of the modern individuated self is an isolation from the surrounding world. For example, modern western medicine treats people in both physical and mental isolation from the world. In other words, illness (mental or physical) is conceptualized as a predominantly personal experience and problem. Furthermore, individuals become marked by these illnesses in such a way as to be made unique. Outdoor recreation participates in similar processes of individuation while also assuming as its target this individual self. In OR, this modern human subject has been studied in terms of motivations (what drives someone into the wilderness), constraints (what blocks people from participating), and the benefits that ensue from the wilderness experience. It is not surprising, therefore, that when OR adopted a similar conceptualization of the self, it also adopted the distinction between the individual and the natural world.

The shared conceptualization and treatment of the individual in OR and other modern disciplines aids in understanding how OR has grappled with problems of paradoxes as well as why the discussion of ecological theory and knowledge is so sparse. The modern separation of self from world is itself paradoxical, and while OR has identified this paradox its response has been constrained by some of the very disciplinary forces that gave rise to the paradox in the first place. Stepping outside these discursive constraints requires a shift in the way OR has traditionally formulated questions about the wilderness experience: who has it, what it means, and where it happens. Ecological literature does not figure as centrally as it might in OR partly because of the emphasis that OR places on individuals. When the focus is on user preferences, benefits to people, wilderness experiences, or motivations, the natural world appears, if it does at all, as the backdrop that enables the multitude of benefits, experiences, constraints, motivations, and preferences.

The shared approach to the individual that science and OR manifest is due in part to the fact that most modern disciplines share a common grid of specification. According to Foucault, one of the rules for the formation of objects in discourse is the grid of specification. Although Foucault spoke about madness and psychiatric discourse, because of the similarities between OR and other modern disciplines, his comments can be applied to outdoor recreation. Grids of specification, then, "are the systems according to which the different [kinds of backcountry experiences, benefits, and motivations] are divided, contrasted, related, regrouped, classified, derived from one another as objects of [outdoor recreation] discourse."[101] This grid performs a division and evaluation in a way that reflects an individualistic emphasis. The benefits derived from wilderness experiences are largely individualistic, and the constraints users face, such as money and expertise, operate on the individual person. Thus is the individual divided out from the aggregate and the human from the nonhuman. Furthermore, there is an evaluative stance that places preference on the individualistic manifestations over the social or nonhuman. That is, most often OR deals with people as separate from nature and the strongest emphasis seems to be on individual people, not groups.[102]

It is fitting, then, for Beringer to remark that the dominant conceptualization of the self in OR is the same as in science: the atomic, autonomous, and isolated self. Of the benefits that wilderness experiences can provide, Beringer continues, the majority are psychological.[103] Psychological benefits of wilderness experiences include developing self-awareness, self-confidence, and self-esteem. Morton summarized these benefits when he stated:

> [w]ildland recreation results in a variety of individual and social benefits including: personal development (spiritual growth, improved physical fitness, self-esteem, self-confidence and leadership abilities); social bonding (greater family cohesiveness and higher quality of family life); therapeutic and healing benefits (stress reduction helping to increase worker productivity and reduce illness and absenteeism at work); and social benefits (increased national pride).... Wilderness is a place for spiritual experiences and has inspired the creation of art, photography, literature, poetry and music. Wilderness is also a place to restore mental and physical health, stimulate creativity, achieve self-realization and improve group leadership skills.... Wildlands provide current and future generations of Americans with a frontier-like environment to reclaim their cultural identity and feed their soul.[104]

---

[101] Foucault (1972a), 42.

[102] Although group research has been a longstanding valuable topic in OR, even in it the group is often taken as a collection of individuals and their preferences, behaviours, and experiences.

[103] Beringer (2004).

[104] Morton (2000), 2. See also Priest and Gass (1997) and McAvoy and Dustin (1989).

Although Morton does list social benefits, the majority of the specific benefits he cites target the individual. It is not uncommon for individual benefits to outnumber other types of benefits. Even economic benefits can come second to personal benefits: the various "benefits of conserving remaining wilderness only adds to the economic benefits, and may be even more important in rallying support for particular areas."[105] Lost in this quotation is the environment itself. Kromos and Martin contended that wilderness should be conserved for the benefits it provides and these are predominantly individual human benefits. Given the empahsis on individuals, it makes sense that the focus of research would be less on ecological details and more on the psychological or even economic benefits of OR participation. There are some who discuss nonhuman benefits of wilderness (for example, benefits to the animals that live there), but these are by far the minority.[106]

To understand these benefits and even help foster them, scholars developed different models (for example, extraordinary experience, flow, and the Adventure Model[107]) that embody experiential qualities such as clear focus and extreme concentration; merging of action and awareness; spontaneity of action; personal control and awareness of power; intense enjoyment; risk and challenge; and even transcendence of self. These are additional benefits, as Ewert and Hollenhorst noted:

> [g]oing beyond the traditional set of benefits ascribed to leisure experiences (e.g., physical exercise), adventure and wilderness experiences have both been described as a means to crystallize selfhood through personal testing, provide life meaning and perspective, confer

---

[105] Kromos and Martin (2003), 8.
[106] For example, Carter (1997) and Cole (1995a).
[107] Ewert and Hollenhorst (1997).

> awareness of one's own mortality, reduce anxiety, and improve fear-coping.[108]

It is clear that all these benefits and models are premised on a self-contained, autonomous individual. As Foucault has commented, the more power is applied to someone, the more he or she becomes differentiated. In other words, power makes people into unique individuals worthy of study. In OR literature, the benefits accrued from participating in wilderness experiences help make people into individuals as they are divided out into varying levels of self-reliance, competence, self-efficacy, cardiovascular fitness, strength, endurance, skills, knowledge, motivation, and self-actualization. While the purpose of OR is not the formal classification of individuals as it is in the school or medical system, the benefits listed in the literature do provide an informal basis for assessing individuals.[109] The surveillance techniques discussed earlier, such as the LNT exam, assist in the process. Certification, permits, and practices are all means of distinguishing an adept user from a novice.

The type of benefits thought to derive from participating in OR has remained fairly consistent throughout the last thirty-five years. Early benefits research cites results similar to those in the more contemporary studies. For example, in 1970 Marsh noted that the wilderness experience "comprises a mixture of feelings such as peace, solitude, surprise, fear, and communion with nature."[110] These are all psychological benefits that accrue to individual selves. The detail and sophistication has developed over the years, with researchers citing more complex mechanisms for and subtle nuances in the development of benefits. There is also a much greater understanding of the variables involved (for instance, ethnicity, gender, class) and how these interact with the activity, the setting, and the intensity. Nonetheless, it is still the case that most of the benefits discussed in the literature remain individualistic (for instance, physical, psychological, or affective).

---

[108] Ewert and Hollenhorst (1997), 21.

[109] For further discussion of the various benefits one can derive from participating in outdoor recreation, see Klint (1990).

[110] Marsh (1970), 124.

That the wilderness experience is seen as providing mainly psychological, physical, or affective benefits to individuals should not be surprising. Part of the workings of the disciplinary technologies in medicine, education, the military, and the penal system is the differentiation of individuals, the separation, evaluation, and marking of them for correction or promotion. When these technologies are employed in OR the same results occur. In the minimum impact literature, for instance, individuals are made more distinct (there are those who stay on the trail and those who diverge off-trail in order to circumnavigate wet areas); they are separated from each other into small groups (campsites are isolated and lines of sight between them are obscured; trails twist and turn); they are evaluated (in terms of one's knowledge of LNT principles); and they are ranked against each other (users who engage in non-motorized activities are usually seen as better while those who prefer quads are frowned upon). Overall, the literature on benefits produces the individual as a discrete and unique entity (for example, one who values solitude or one who has a spiritual experience).

By way of summary, there are some disjunctions between the stated goals of OR and its discursive practices. These are most apparent in the way that OR has emphasised other aspects over ecological knowledge, and the way that it has worked more fully to individuate users and then classify and organize them. As a field, OR has already recognised the importance of preserving wilderness areas and initiated a program designed to protect them. However, as laudable as this goal is, there is still room for improvement. For example, one of the main aims of the LNT curriculum is to be conscientious of other users, which, once again, shifts the attention from ecology to human factors. In terms of benefits, the idea of communing and connecting with nature is not easily compatible with research that stresses the development of benefits for individuated human beings. In fact, promoting and focusing on the modern self isolates that self from the natural world. In addition, OR has also had to contend with some troubling paradoxes. Specifically, these can be seen in the debates surrounding freedom and regulations as well as authenticity and change.

In some ways these paradoxes have polarized the discussion into an either/or situation where one side of the binary must capitulate to the other. Thus, freedom is accomplished only with the removal or reduction of regulations. The push toward seeing wilderness as authentic and original greatly complicates the idea of change. When certain changes are allowed in wilderness, the possibility of destroying that wilderness arises. As a result, the idea of change becomes precarious: it is both natural and necessary and disruptive and unnatural. These paradoxes have proven highly resistant to OR's attempts to resolve them. The next chapter outlines in more detail what is perhaps the most foundational of the paradoxes that ecology and OR have both encountered and produced: the separation of culture from nature. One of the purposes of the present chapter was to illustrate the ways in which OR has furthered the individuation of the modern self that is then conceptualized as separate from nature. From this more specific manifestation, we can then see how ecology and OR are both premised on a general separation of culture from nature.

CHAPTER 4: HUMANS AND NATURE

The machine does not isolate man from the great problems of nature
but plunges him more deeply into them.
~ Antoine de Saint-Exupéry, 'Wind, Sand, and Stars'

This chapter expands on the idea, introduced in Chapters Two and Three, that the most significant discursive practise in ecology and OR is the separation of humanity and nature. The first of the two sections in this chapter provides a brief outline of the history of this split. The second section discusses three techniques used by ecologists and OR scholars to attempt this split as well as some complications that arose during these attempts. The history of the human/nature split could begin deep in the roots of Western Euro-North American culture. Our story, however, will commence with the development of science and the scientific method in the Enlightenment, along with developments in philosophy and social contract theory that reinforced the faith in rational logic and the power of human ingenuity both to understand and to manipulate the world. The developments of the Enlightenment provided the basic architecture for modernity, within which both ecology and OR are embedded. Rational, inductive science and faith in human intellect to solve problems with the application of logic are two of the hallmarks of modernity that stem from the Enlightenment. Understanding these developments and their implications for ecology and OR are the goals for this section.

Appreciating how ecology and OR arrived at the tenuous position of searching for natural nature to study or recreate in necessitates an exploration of the links between science, rationality, modernity, and the human/nature split. The more formal emergence of ecology at the turn of the 1900s marks it as a latecomer in a long process in the development of modern scientific disciplines (for example, chemistry, biology, medicine, and physics). While OR can be classified as social science not a physical or natural science, many of its experimental techniques, statistical analyses, data collection methods, and laboratory procedures are similar. One of the goals for ecological research was to identify and understand pristine ecosystems or, when that proved impossible, identify the effects of human interference and then subtract this from the models of ecosystem processes. Excepting studies of human-created ecosystems (for instance, agricultural or radiation studies), ecology separated human activity and impact from the systems it studied. In OR a similar search for pristine, natural nature occurred. In many ways, natural nature was even more important for wilderness advocates because without it there would be no backcountry recreation. In all the writing on minimum impact and leave-no-trace ethics the desire for unaltered nature runs high. Of course, unaltered nature only occurs where humans do not alter it. In this way OR replicated and reinforced the notion that people and nature do not mix.[1] The first section of this chapter is devoted to examining the historical details of this separation.

The second section of this chapter indicates three ways this split has manifested in ecology and OR. These are patterns in the discourse; they are not the result of an individual's actions or publications. In fact, it might be more accurate to say that the individual examples I use are themselves the product of the discursive patterns rather than the producer those patterns. My use of specific examples should be understood, then, as an illustration of the larger and more general discursive pattern. This is the difference between wondering how it was that so many scholars in OR produced works with certain similarities as opposed to criticizing these scholars for not critiquing the discursive patterns in OR. When these discursive forces remain unexamined, normative patterns can be replicated. So, for example, division and rejection is a common discursive practise that finds a specific manifestation in OR literature as a normative separation of humans from natural nature. When this pattern remains unseen, and therefore unexamined, the consequences of it cannot be evaluated. There are three basic aspects to the pattern of separating humans from natural nature that I wish to illustrate below: shifty/ing discourse, scare quotes, and paradoxes. In particular, I will focus on the unintentional elements within these aspects. I contend that shifty/ing discourse and paradoxes are most often unintentional; that is, authors' texts perform this shift/shiftiness and invoke paradoxes and, yet, nowhere in the literature do authors comment on either the reason or consequence of this performance. The case of scare quotes is somewhat different. The use of these quotes seems to be more intentional; however, scare quotes are presented as self-evident, as if the meaning was unambiguous and obvious. I argue the opposite: the use of scare quotes indicates a very complex layering of meanings. Furthermore, some of these meanings are contradictory. In the end, scare quotes confuse, more than they clarify, the meaning of the text. Given that scare quotes are often placed around central concepts, such as *nature* or *pristine*, I suggest that the resulting confusion is not intentional

---

[1] For example, the LNT ethic argues that wilderness users "just need to make [their] passing as undetectable as possible" (McGivney 1998, 8).

In the first case, shifty/ing language appears in OR and ecological discourse even as the texts proclaim a singular focus on natural nature. In almost all instances where ecologists and OR scholars speak of pristine nature, they have in mind places where the influence of humans is absent, remains minimal enough to be discounted, or has been removed (that is, wilderness restoration). However, in many of these cases, what actually occurs in the discourse is something different. The claim may be that the pursuit of pure wilderness, devoid of all traces of humanity, is the goal, but this position is not maintained internally consistently. The discursive concept, *natural nature*, is nearly always arranged in close proximity with another discursive configuration: the non-natural human. This inability to enunciate *natural nature* without also invoking *non-natural human* and then conflating the two at some level marks one problem with the attempt to split humans from nature.[2]

The second problem arises when researchers attempt to define what it is they are studying. It is one thing to state that wilderness is a place where evidence of human influence is absent; it is another thing altogether to explain what exactly this entails. As noted above, discussions of nature can be problematic. The reason for this, I argue, lies in the problem of clearly defining natural nature. This difficulty manifests in a strange fashion. Often, when words or phrases such as *nature*, *natural*, *wilderness*, *ecological balance*, *natural state*, and so on occur, they are placed, rather ambiguously, in scare quotes.[3] The reasons for and consequences of scare quotes form the second aspect of the pattern of separating of humans from nature.

The third aspect to be discussed in this chapter is the paradoxical nature of the split between humans and nature. In actuality, one might say that the first two aspects are but illustrations of the paradox that gets invoked when nature is described as distinct from people. This paradox, unfortunately, is not recognized overtly or accurately. Denial marks the most common approach to the paradox of nature/humanity: scholars simply place these problematic terms in scare quotes and move on. The other solution (that really is no solution) is to misunderstand what a paradox is. In these cases, writers will point to the many paradoxes they see in ecology or OR that actually are not paradoxes. Interestingly, these pseudo-paradoxes do not raise problems that are as difficult to deal with as actual paradoxes. Even if we can only speculate as to the reasons why the more troubling paradoxes are avoided, we can comment on the results of side-stepping the more difficult, but accurately conceived, paradox of nature by calling attention to these pseudo-paradoxes. The purpose here is only to give some examples of the denial of paradoxes as well as the types of paradoxes researchers suggest are present. Discussion of the more troubling kinds of paradoxes is the topic of Chapter Seven, while Chapter Eight begins to construct an alternative to denial for addressing them.

---

[2] For example, see Christensen and Clark (1983).

[3] Scare quotes are single quotation marks. See Chapter One for a brief discussion of this concept.

Modernity and the Enlightenment

> *If you would be a real seeker after truth, you must*
> *at least once in your life doubt, as far as possible, all things.*
> *~ René Descartes, 'Discourse on Method'*

When discussing the history of the human/nature split in modern science, the Enlightenment and the concomitant broad philosophical and social changes mark a moment of significant change. One of the most important changes was the foregrounding and development of science and rationality. The discursive practises in ecology and OR that were outlined in Chapters Two and Three were pre-dated by many similar developments in the Enlightenment. Practises around the organisation and classification of information, for example, took on a more scientific configuration. The concern in ecology over qualitative versus quantitative data bears marks of the earlier development of scientific practise in the Enlightenment. Testability, verification, prediction, control, manipulation, reliability, and generalizability all became central features as science developed, and both ecology and OR manifest these today. Causal explanations were sought in science as they are today in ecology and OR. The relations between statements and objects were explained in terms of physical and functional likeness. For instance, classification systems were developed that emphasized how organisms were physiologically related to each other.

Furthermore, as the scientific method evolved, different criteria came to the fore indicating the place, purpose, and power of different statements. Scientific thinking classified statements according to their objective truth value. That is, some statements were false (for example, lies or make-believe), some were neither true nor untrue because they had yet to be tested (for instance, hypotheses), others were true given particular conditions and constraints (for example, the law of density of gases only applies under conditions of constant temperature), and, finally, a few reached the level where the conditions and constraints on their veracity were so minimal they were taken as absolutely true (for instance, mathematical formulations).[4] Part of the shift that occurred in the Enlightenment was a switch from why questions to how questions: How does nature work? How do animals behave? How do things affect one another? Logical and mathematical thought became paramount. These are some of the conditions for verification that Foucault spoke of as indicating the threshold of scientificity had been crossed.[5]

One figure who shaped the future practises of ecology and OR would undoubtedly be René Descartes, who "believed that, whereas he *was* a mind, he *had* a body. The two were radically distinct from one another."[6] Reason is not dependent upon nor affected by physiology in Descartes' world view. Thus, he proposed that substances were either extended into the physical plane (*res extensa*) or internalized and not extended (*res cogitans*): physical substance and thinking substance. Descartes placed more emphasis on *res cogitans* because true knowledge came only from rational thinking, not sensing.[7] Under this conceptualization, mathematics held a privileged position in the hierarchy of statements because it depended so little on the senses. Consequently, Descartes was convinced "that science based on mathematics was the only way to reach such certainty."[8]

---

[4] See Toulmin (1960) for a more detailed discussion of the hierarchy of scientific statements.

[5] Foucault (1972a). For more discussion of the changes inaugurated during the Enlightenment, see Chapter Two in Bowler and Morus (2005); Conley (1997); Derrida (1978a), 31-63; Dreyfus (2004); Latour (1999 and 2004); Chapter One in White (1998); and Chapter Sixteen: The Wealth of Nature in Worster (1993).

This has become known today as Cartesian dualism: a split between mind/body, objective/subjective, rational/sensual, and true/false. The emphasis on internal versus external matter led Descartes to a rather extreme position with regard to nature. He felt that animals, because they consisted only of *res extensa*, were essentially automatons or chunks of moving matter; they could not be a *Cogito*. Howell explains the connections and implications of this:

> [i]n western moral philosophy since Classical times, humans have been set apart from—and above—all other animals according to some essential criteria.... This separation has been justified on grounds of moral superiority and reinforced by the Cartesian separation between mind and body, associated with thinking and feeling respectively. The properties of these dualities have not been held to be of equal value: humans are superior to animals, mind is superior to body, just as thinking is to feeling.... When we further consider a dominant strand of thinking which holds that mind is cultural and body is natural, we find ourselves within the familiar western schema.... It is ... an approach which has

---

[6] Belsey (2002), 66.
[7] See Hargrove (1989) who noted that early scientists "followed the mainstream Greek position, concluding that sensation interfered with the study of nature, [thus] they tried to look beyond the world of experience [the sensual world] to find principles that applied to ultimate reality in its most fundamental form." 38.
[8] Prigogine (1996), 185.

> universalistic ambitions and it has proved peculiarly resistant to challenges.[9]

This powerful, dualistic mode of thinking buttressed the scientific method's privileging of rational, objective, logical thought (*Cogito*) directed toward an independent and dispassionate external world (*res extensa*).

From the Enlightenment, then, comes deeply embedded ideas of rationality, a mechanical or predictable universe of matter, and free will and progress. These ideas made it easier to see nature as merely a resource to be used in the quest for progress and improvement. In fact, nature was not just used to further progress; the use of nature was a mark of progress itself and was seen as an improvement over the older idea that nature, as a divine creation, was impenetrable to human logic and understanding. The system used to produce statements, concepts, theories, and other objects of discourse that developed in the Enlightenment has remained quite constant in its emphasis on rational and logical deductive argumentation, and statements in ecology are produced and arranged today according to a very similar archive. That is, ecologists often made it clear that they were not creating subjective or personal narratives about nature. Instead, ecologists tied their project to the larger, longer lasting, and more powerful discourse of science that had grown since the Enlightenment. The archive, which these sciences drew from, set out forms of verification, established relations between speakers and knowledge, required certain practises and habits, and existed in various relationships with other discourses and discursive practises.

Over time, the archive that produced statements in science established some speakers as more legitimate than others. Those schooled in the techniques of scientific data collection and analysis were able to enunciate and circulate propositions and other discursive objects that were seen as disconnected from the individual who uttered them. In other words, at the same time as scientific discourse was hiding the individual behind notions of the dispassionate observer status, an objective orientation toward knowledge, and rigorous methodology designed to remove bias, it was also creating a privileged subject position from which only qualified individuals could speak about nature with authority. The debates in ecology in the early decades of the 1900s over quantitative versus qualitative data speak to this ongoing quest for legitimation. By connecting with the established discourse of science, ecology was more easily able to position itself as an authority on matters of natural systems and processes. When outdoor recreation crossed the threshold of positivity and strove toward the threshold of scientificity during the 1960s/1970s, it too turned toward scientific discourse as a means of purchasing some of the cultural power science had to craft statements and objects that were then seen as independent of such crafting. In both ecology and OR the archive from which discursive objects were produced had much in common with a practise that extended back to Descartes. The requirement that statements should be susceptible of testing and verification, independent of the tester, sets science apart from other endeavours, such as painting, where the author/artists is seen as a unique individual who exhibits exceptional skill. In science, the individual is less important as a figure. In fact, the mark of a better scientist is the absence of personality in the experiment and its conclusions. Good experiments were ones that could be recreated by others and the same or similar results should occur because the person who conducts the experiment and the subject of the experiment were split apart in the Enlightenment. In terms of the natural sciences, this resulted in a separation of the natural world from the human world or nature from culture.

---

[9] Howell (1996), 127.

Unfortunately, the attitude that nature was something separate from humans and should be put to use in service of development and social reform was not an innocent or simply emancipating change. Conley points to Lévi-Strauss who

> calls into question the separation between nature and culture, between body and mind, and he especially rules out the concept of a static condition of nature. Without denying the existence of a 'real,' he shows how it is always organized through language that changes no less than the world it is said to represent. It follows that different ideologies, or ways that language imagines the world, have differing impacts on nature. And the ideology upon which the autonomous, masterful subject was founded in the post-Cartesian era has proven disastrous to many humans and nature.[10]

From this root, ecology and outdoor recreation have continued to produce their discursive objects through an archive that views humans and nature as separate. Ideas of progress, and of nature as fundamentally separate from humans, form conditions of thought in both ecology and OR throughout most of the twentieth century.

This is not to say that ecology and OR maintained this separation to the exclusion of all other perspectives. As ecology developed over the twentieth century other forces entered into the picture and challenged the neutrality of the scientists. Ecologists such as Eugene Odum began in the 1950s to include more subjective and value-laden perspectives in their work. Odum began to include more and more environmental commentary in his textbooks over their three-plus decades of publication.[11] He also began, in the 1980s, to write other books that more explicitly contained an environmental message. For instance, even the title of *Ecology and Our Endangered Life-Support*

---

[10] Conley (1997), 42-43.

[11] Compare the first edition, Odum (1953) with the last Odum (1993).

Systems, published in 1993, suggested a linking between human society and nature. Odum's inclusion of a more subjective and value-laden perspective was not uncommon from the 1980s onward and reflects the uncertainty surrounding environmental interpretations that Watts and Peet note became more and more common in the 1980s and 1990s. These authors recognized that when understandings of environmental issues are unclear, as happened when ecology began to realise nature's complexity, experts are required to make sense of the problem. However, these experts "are more important to the political solution than the content of the ideas *per se*."[12] This illustrates the development of new and more powerful subject positions in ecology that were a combination of the detached and objective scientific position with the engaged, committed environmentalist one. Interestingly, these positions relied less on the content of their knowledge than they did on the position from which individuals could speak in meaningful ways about the environment.

In OR, too, a similar process was underway. Early works, such as Jubenville's 1976 textbook, *Outdoor Recreation Planning*, and the edited conference proceedings, *Elements of Outdoor Recreation Planning*, by B. L. Driver rarely preached about environmental issues.[13] Instead, these and other books of the time discussed topics such as supply and demand, state level planning, user preferences, and the nature of planning for outdoor recreation experiences. Classification schemes that arranged landscapes into types in which only certain activities were allowed (for example, non-motorized activities only in more primitive locations) were beginning to appear and attention was directed toward how best to manage areas consistent with the stated goals for that type of area.

---

[12] Watts and Peet (2004), 22.
[13] Jubenville (1976) and Driver (1970).

This neutral position vis-à-vis environmental issues soon changed however. Over the next few years, OR researchers quickly realised the potential outdoor recreation had to destroy the very areas it utilized. More and more authors began commenting on the dangers of unrestrained OR in wilderness areas. Moreover, impassioned pleas for more protection and reconstruction of wilderness areas surfaced throughout the 1980s and into the 1990s. Today, it is difficult to find textbooks and monographs that do not at least mention some of the troubling implications of rates of high wilderness use. These types of commentaries, which crossed the scientific barrier and entered into more philosophical or in some cases religious terrain, represent the production of a new set of subject positions in both OR and ecology. Researchers now spoke not only from a scientific paradigm; they also spoke unapologetically from an ethical one.

The connection between the Enlightenment and modern science show the power and persuasiveness of Cartesian dualism and the split between humans and nature. However, even though this split is framed as a requirement for truth claims in natural science, it has not been easy to obtain or maintain a clear separation. Although there is a long history to Cartesian dualism and one would suppose that the conceptual, actual, and textual separation of humans from nature would be an accomplished fact by now, humans actually occupy a difficult and somewhat ambiguous position in ecology and outdoor recreation vis-à-vis nature. The next section investigates how difficulties in maintaining a clear distinction between humans and their culture on the one hand, and nature in its pure and wild form on the other hand periodically arose in both ecology and OR. The ways in which ecologists and OR scholars engaged the problem of keeping nature out of culture are instructive in many ways. First, they show some of the excesses of discourse and slippages of meaning that cannot be contained by the text. Thus, even when authors try to limit or contain their text, meanings disseminate outward. So, for example, when attempts to focus on nature slip back onto a human-centred focus, we can see the inherent excess of this divide that threatens discursive boundaries. Second, the various attempts to keep culture and nature separate have resulted in interesting and revealing textual practises. In particular, in both ecology and OR single quotation marks are used around specific words as a means of pointing to, accommodating, or addressing the slipperiness of concepts like nature, wilderness, pristine, and authentic. Third, these difficulties highlight the prevalence of paradoxes in both fields and the problems these paradoxes presented.

The Human/Nature Split in Ecology and Outdoor Recreation: Where Did Everybody Go?

None shall pass!
~ Tim The Wizard, Monty Python's The Search for the Holy Grail

The idea that people and nature are separate stretches at least back to the Enlightenment and is entrenched in the operation of and assumptions within science in general and ecology in particular. In ecological studies, this manifested as an emphasis on the interactions and relations between natural components that did not include people. That is, what ecologists most often took as a natural system did not include humans or evidence of human influence. Thus, quadrats were mapped out not over people's houses or roads, but over fields and forests. With outdoor recreation, similarly, the idea that wilderness was (or could/should be) people-less and pure reflected this divide. Volumes of studies in OR have been conducted on the problem of keeping these places in (or restoring them to) their original wild state. The suggestion that ecological and OR discourse focused on and privileged natural systems may seem obvious and straightforward. However, it is necessary to state the obvious because obviousness itself contributes to the problem here. The idea that natural nature excludes people receives little critical attention when it forms conditions instead of objects of thought. The purpose of this section is to raise this premise to the level of object of thought in order to initiate the process of developing alternatives.

When the premise that nature and humans are distinct is carefully considered it quickly becomes evident that this presumption is anything but simple and clear. There are times when this lack of clarity is commented on by researchers; however, in the main, it is an accepted presumption that is contained within the operation of such basic terms as *wilderness*, *pristine*, *authentic*, *natural*, and so on. What is particularly interesting is the way in which the disciplinary pressures for coherence, logical structure, and persuasiveness collide with the use of these terms and concepts. Defining what they meant when ecologists spoke of a natural ecological system or what outdoor recreation referred to when discussing wilderness forms the site for the collision between the modern disciplinary regime, manifested largely as a scientific project, and the inherent contradictions invoked by the human/nature split. In other words, as modern discourses, ecology and OR are evaluated according to certain standards: they should be rational, coherent, systematic, rigorous, and persuasive; yet, when these criteria are applied to central concepts like natural and wilderness, the discourse does not/cannot measure up.

The location of this encounter between the demands of scientific discourse and the deployment of central concepts of nature and culture is a fertile place to begin the search for an alternative framework. At this location, for example, the slippage of meaning becomes evident. Even as authors emphasize their interest in natural nature, discussions nevertheless often slip into discussions of humans and human interests. In other cases, authors clearly recognize the instability of the human/nature divide, and have developed a strategy to accommodate, contain, and/or acknowledge this slippage and excesses. In these situations, scare quotes (single quotation marks) are often used to mark these words as problematic. This textual strategy is revealing because it suggests an awareness of the instability of the human/nature divide and yet does not resolve the issue in a substantive manner. Both the shifting discussions and scare quotes are symptoms of the paradoxical nature of this problematic. Many authors have commented on the paradoxes in both ecology and OR; however, more needs to be done to address the problem. In short, the collision between science and the re-inscription of the human/nature split offer three points of engagement: (1) the slippage in meaning of certain key concepts, (2) the use of scare quotes as a means of containing or addressing this slippage, and (3) the paradoxical nature of this problematic. The remainder of this chapter is devoted to discussing these three points.

It's All About Nature?: Understanding Shifty Nature

> *We have no right to assume that any physical laws exist,*
> *or if they have existed up to now, that they will*
> *continue to exist in a similar manner in the future.*
> *~ Max Planck, 'The Universe in the Light of Modern Physics'*

Outdoor recreation has borrowed not only models from ecology, but also the same power-knowledge structure of science. The requirements for truth within the discursive rules of OR thus emulate those of ecology: scientists in outdoor recreation were searching for an autonomous factual nature that may change, but did so of its own accord (that is to say, naturally). Interference of humans in this process constituted just that—interference. Nature could not be natural when humanity interfered. It is worth looking more closely at the role that natural nature has played in OR discourse as well as the overt attempts researchers made to discuss a nature distinct from humans, for these instances illuminate part of the paradox of the human/nature split.[14]

The Journal of Leisure Research illustrates the occurrence of ecological terms and themes in outdoor recreation research. *The Journal of Leisure Research*, one of the top journals in recreation and leisure studies, has published articles on environmental and natural landscape preferences,[15] the differences between public and professional definitions of state parks,[16] evaluations of aesthetic qualities of trees and other elements in wilderness areas,[17] the demographics of conservation groups,[18] outdoor recreation planning objectives,[19] the carrying capacity of wildland recreation areas,[20] the concerns over wilderness crowding,[21] the differences between Native American and European wilderness use and values,[22] wilderness recreation and ethnicity,[23] and suggested a number of different models for conducting research on outdoor recreation.[24] In other journals, articles addressing environmental concern have been appearing since the early 1970s. In many of these, the focus is on questions of whether participation in OR can increase a person's environmental concern. For example, in 1975 Dunlap and Heffernan proposed that participation in OR would increase a person's environmental concern. Their second hypothesis was that activities that were appreciative instead of consumptive (bird-watching instead of hunting, for example) would lead to more environmental concern. Their final hypothesis stated that environmental concern would be strongest for aspects of the environment necessary for participation in the OR activity. Thus, canoeists', rafters', and kayakers' concern for air pollution would not be as strong as their concern for water quality.[25]

---

[14] It must be noted here that one should not conclude from this that nature and human are somehow joined together. In Chapters Six and Seven I argue that it is not a collapse of the line between nature and humans I am seeking but, rather, it is a backward erasure of it. We need to learn to think the time before the separation was attempted, not the time after they were joined back together. Linking them back together suggests that they were successfully separated. It is precisely the logic that takes these two terms as items to be rendered asunder or merged together I wish to challenge.

[15] Borrie (2001), Shafer, Hamilton Jr., and Schmidt (1969), Shelby, Vaske, and Harris (1988) and Whittaker and Shelby (1988).

[16] Merriam Jr., Wald, and Ramsey (1972).

[17] Cook Jr. (1972) and Shelby and Shindler (1992).

[18] Harry, Gale, and Hendee (1969).

[19] Hill and Shecter (1978).

[20] Greist (1976) and Harshaw and Tindall (2005).

[21] Manning (2003). See also the interesting and heated discussion between Crompton (2002), Driver (2002), Dustin (2002), and More (2002a and 2002b).

[22] McDonald and McAvoy (1997).

[23] Johnson et al. (1998).

[24] Burdge and Field (1972).

With all this discussion of the environment and environmental issues, one would expect there to be an emphasis on natural nature (especially given that many authors overtly emphasized attributes that comprise a wilderness setting). However, a closer examination reveals that most articles are quite light on ecological theory, and even when engaged in discussions of pristine wilderness, tend to shift onto discussions of human use, human preferences, human experiences, and human motivations.

At least one OR scholar has expressed concern for this slippage onto human concerns. In 2002, Cole commented on a workshop in which OR researchers discussed the concept of visitor crowding. He noted that by and large the discussion invariably returned to questions of visitor experiences not to ecological concerns: "discussion was largely confined to situations in which concern for the visitor experience is the basis for use limits, since this is more controversial than limits based on ecological impacts."[26] By this he meant that once visitors are convinced of the presence of an ecological rationale for limiting use, they are much more likely to accept that limit. It is telling that even when a researcher notices this slippage and expressly invokes ecological concepts, the complexity of ecological concepts does not also appear. It may be the case that visitors more readily accept ecologically based limits; however, this does not mean that the concepts used to base the limit on are not themselves controversial. In this way the discursive rules in science operate to divide and reject the controversy so as to present a more unified and coherent position. The disciplinary pressures to legitimate knowledge through science marginalize discursive objects that are unclear, problematic, or even contradictory. In ecology, debates over the meaning and use of models and concepts that OR drew from were commonplace. However, in OR these debates have very nearly disappeared. In OR the problem was not ecological (or with ecological terms, models, and concepts); it was a problem with people.

---

[25] Dunlap and Heffernan (1975).
[26] Cole (2002), 19.

The article by McIntyre and Roggenbuck is representative of this shifting.[27] They investigated five types of foci for students' attention on a wilderness trip to a cave: (1) focus on nature as place or object, (2) focus on self and internal thoughts, (3) focus on others, (4) focus on emotions and affect, and (5) focus on task or activity. Although the authors assert that these five modes of interaction are directed toward experiencing nature, there is a difference between focusing on one's own feelings and focusing on nature as a place or object. This distinction gets erased when all types of foci are framed as being about nature. I am not suggesting that a focus on humans is wrong or unhelpful. Instead, I am suggesting that the way this slippage occurs is important, as is acknowledging its existence. In part, I argue that the slippage is a product of disciplinary pressures that pushed OR into a quest to legitimate itself in the larger arena of publicly funded university programs, a national debate about the role and value of protected areas, and the explosive growth in participation in outdoor recreation activities.

---

[27] McIntyre and Roggenbuck (1998).

I also contend that simply acknowledging this shift is important. When the focus is ostensibly on nature, but the substantive content is not, the question of whether natural nature is at risk or threatened can be hidden. In their article, for example, McIntyre and Roggenbuck discuss the students' focus on nature in terms of amazement at the limestone formations; the mysteriousness of the cave; the sensation that the limestone was watching them; feelings of timelessness and losing track of where they were; feelings of vulnerability and magic; feelings of awe, respect and nervousness; and a sense of being one with nature. In this context, questions of whether the limestone formations were threatened or not, and of how animal populations in and around the cave reacted to the presence of OR participants are more difficult to formulate. When this general pattern in OR forms, the disappearance of questions directly about the health of natural systems becomes much more systemic.

This pattern of shifting or slippage in OR studies is quite common. For example, in 1981, Machlis, Field, and Campbell noted that the biophysical environment provided resources for both natural and social elements. "For example, a forested watershed may function as a habitat for wildlife, a natural area for hiking, a setting for interpretation, and as a source of potable water for park staff and visitors."[28] Of the four functions mentioned, only one expressly referred to natural resources and none of the four indicated whether the forested watershed was threatened, under stress, being altered, or healthy. The other three factors reflect a human-centred focus to the extent that potable water is mentioned only in relation to park staff and visitors, as if animals do not also need potable water. Again, with the shift in focus from ecological factors to human-centred ones questions about the ecological status of the area disappear.

---

[28] Machlis, Field, and Campbell (1981), 200. This slipping is present within the LNT principles themselves as Jones, Hollenhorst, and Tino (2003) illustrate when they note that while there are three LNT key priorities for rock climbing, they are going to focus only on "the second category of impacts to other people," 348. Leave No Trace! principles were started with the intent to address ecological impacts of OR use. However, they soon expanded to encompass a concern for people's experiences and, thus, impacts on visitors became a key component. See McGivney (1998 and 2003) and Turner (2002) for a discussion of the LNT program.

This slippage has been part of OR discourse since the early 1970s. In 1970, Marsh classified wilderness areas at the opposite end of the spectrum from cities; the former had largely "retained a character almost unaltered by man."[29] Marsh was one of the earliest Canadian authors to recognize the impacts OR could have on wilderness and to argue for its protection. He posited that the purpose of national parks should be to safeguard wilderness areas in order "to provide most wilderness users with a satisfying high-quality experience."[30] In an outdoor recreation conference in 1985, Curt Shirer argued that because more and more people are using the wilderness, we need to consider "the future of the resource and the participant.... This may be done by avoidance of excessive equipment and by complementing outdoor adventure activities with related activities.... The goal should be the long-term maintenance and enhancement of the program participant's life."[31] In these examples, what began as a discussion of the need for conservation and the mitigation of OR impacts, ended with a focus on the quality of human experiences. The concern for the quality of the wilderness ecosystem disappears and is replaced by a concern for the quality of user experiences.[32] In a sense this is not surprising. In 1995, Hendee reported on a study he conducted in the early 1980s, which examined the numbers and content of academic courses that dealt with wilderness. In the 1980-1981 academic year "only 28.5 percent of the courses were focused on the science-based topics of wilderness protection and management or wilderness as natural ecosystems. Wilderness appreciation and use and wilderness allocation and classification accounted for 43.7 percent of the course topics with history and environmental education/ethics accounting for most of the rest."[33]

---

[29] Marsh (1970), 124.
[30] Marsh (1970), 124.
[31] Shirer (1985), 87.

The very wording used in OR often indicates the proclivity of the discourse to shift from ecological concerns to human ones. The word *resource*, for example, is a common term used to describe wilderness areas. Attarian, for instance, noted with concern that "because of this growth and interest [in OR between 1987 and 1997], the natural resources that support these activities are being compromised."[34] The emphasis here is not solely on the environment. Calling natural nature a resource entails use and activity. It is a resource for something or someone.[35] When Hendee and Dawson discussed the top seventeen threats to wilderness resources, they included many threats that concerned the experiences of users more than the health of the ecosystem. For instance, aircraft flyovers "cause noise and visual pollution, and dilute solitude with a dramatic reminder of modern society to which wilderness users object."[36] Even in cases where the concern is more clearly the condition of the ecosystem the emphasis on users still emerges. According to Hendee and Dawson, livestock were a threat both because they trample and graze the native vegetation, but also because they detract from user experiences—seeing cows or horses is not part of the wilderness experience. Likewise, air pollution is a serious threat to wilderness because, obviously, it has ecological consequences; however, "the accompanying reduced visibility … may impact wilderness experiences" and has caused visitors to change their trip plans.[37] When we call natural nature a resource, we are implying its use by people and our language shifts from concerns for the natural environment to the experience and expectations of people using that environment.

---

[32] See also Bell (1997), Borrie (2001), Christensen and Clark (1983), Daniels and Marion (2004), Drury and Bonney (1992), Laven, Manning, and Krymkowski (2005), McAvoy (1990), McGivney (1998), Miles (1990), Shafer, Hamilton Jr., and Schmidt (1969), Strathcona County Parks and Recreation (1987), and Wall (1989).

[33] Hendee (1995), 24.

[34] Attarian (2001), 147.

[35] See also Christensen and Cole (2000), Cole (1997), Cook and Borrie (1995), McCarthy and Dower (1979), and Newman et al. (2003).

[36] Hendee and Dawson (2001), 8.

It is, perhaps, easiest to see this shifting discourse, and the difficulty that it brings with it, in the attempts to define and explain just what is wilderness or natural nature. As noted in Chapter Three, Glover said wilderness was "a place you just let be and let go just by itself."[38] This understanding of wilderness led to his assertion that non-action was best for wilderness. That is, we should leave these areas alone. We should not manage them; we should let them manage themselves. The following is Glover's appraisal of the benefits that would accrue were we to adopt his tactic:

> [t]he ecological results might be less important than what such a notion does for us. For it reminds us that we need not be striving, improving, and controlling all the time and every place. We can accept some places just as they are, live with certain processes without trying to channel them, watch events happen without judging them.[39]

Notice that ecological benefits no longer occupy a position of prime importance. Letting wilderness be, leaving it to itself, is important for anthropocentric reasons. What is particularly interesting about these examples is the absence of acknowledgement of the shift. This absence, I argue, comes as a result of a couple of factors. First, OR as a discipline has its roots not so much in the natural sciences as it does in the social sciences. Relatively few OR scholars are also ecologists by training. The academic training that most receive positions them much closer to the social-psychology perspective than to that of the natural sciences. Second, there is the understanding that situates wilderness as a place in opposition to humans. This understanding blurs the ways in which humans are responsible for the creation and maintenance of wilderness and, thus, obscures the inevitable spectre of the human that crops up in discussions that begin with a focus on natural nature. When the relationship between wilderness and culture is seen as one of distance and distinction, it becomes easier to think in terms of one side or the other without realising how both are implicated and embedded within the other.

---

[37] Hendee and Dawson (2001), 7.
[38] Glover (2000), 7.
[39] Glover (2000), 8.

It is not just in the professional articles and monographs that this slippage occurs. The development plan for Strathcona County provides another example of this shifting discourse. This report justified developing lake management plans because "[t]he County has a very good outdoor recreation resource base. The lakes system is integral to this base, yet many, if not all lakes are in poor condition and should be better managed."[40] The use of the term 'resource' already begins the slipping. Furthermore, undoubtedly 'poor conditions' refers at least in part to the recreational potential: as the natural ecological processes break down, the area shows more evidence of human impact, which lowers the recreational value of the area. In this instance, the emphasis on ecological quality of the lakes shifts to a more human-based concern: the potential of the area for recreational use. In 1970, Cowan also made a similar shift in the course of his discussion of the role of ecology in Canada's national parks. He argued that parks were more than just recreational opportunities; they were also wildlife refuges where rare ecosystems could be preserved in an unaltered state. These areas were important because they "may some time serve man in ways not yet imagined."[41] Again, we see the attention shift from ecosystems to human affairs.

---

[40] Woods Gordon Management Consultants, Richard A Nuxoll Consulting Services Ltd., and MTB Consulting Ltd. (1985), 67.

There are some interesting examples of the shiftiness of the discourse in both ecology and OR that do not follow the pattern outlined above. That is, the focus will often shift, but not always onto humans. In his 1949 article on natural control of animal populations, for instance, Solomon defined the term *natural* with respect to control of the numbers in animal populations. He said it referred to "that regulation of the numbers of a natural population which keeps them within the limits of a more or less clearly definable though often very wide range of abundance."[42] This sidesteps the issue rather than addresses it. Solomon defined control, and devoted the next paragraph to specifying four different ways to understand control, but nowhere did he indicate the role that the term natural played. Presumably, the mechanisms of control are natural ones because they operate in a natural population, but *natural* is precisely the term under question. Using a term in its own definition presents a circular argument. Although *natural* is a key concept in Solomon's article, it is assumed to be self-evident and escapes rigorous definition. It is difficult, it seems, to discuss and define clearly the concept *natural*. This difficulty was apparent over fifty years later when, in his discussion of biodiversity, Bocking noted a challenge: "[h]ow can we make sense of the incredible diversity of life and its countless points of contact with human society?"[43] According to this, life does not include humans: life comes into contact with humans. The idea that humans and nature are opposite is ubiquitous and paradoxical. These two features combine to render particularly problematic the challenge of specifying what exactly is natural and what not. On the one hand, biodiversity should include all lifeforms, but on the other hand, this grouping removes the possibility of speaking about nature as a special and separate place that needs protection from humans and their culture.

---

[41] Cowan (1970), 323. See also Nyland (1970), who noted the degradation of wilderness and argued for a radical change to our management of natural resources in order to "help prevent physical and mental degeneration of the human race" 40.
[42] Solomon (1949), 2.

In a similar manner, when Butler discussed global warming and the devastation that would follow, he noted, with concern, that there would be "serious consequences of rising temperatures for skiing." In the end, because of the amount of recreation that occurs on or near the ocean, "[t]he social and particularly the economic upheaval and the necessary readjustments could result in even greater change in recreational and tourist behaviour in Canada and the world at large."[44] The separation of humans from the environment makes it easier to treat each one independently. Thus, Butler can state that the *serious* consequences are economic ones that will dramatically affect OR without at the same time commenting on the devastation global climate change would cause. In works that textually separate humans from nature, this shift is quite common. Although the separation could expedite the treatment of each term equally, it seems in the end that the emphasis falls on the human side of the dualism. Witness Butler's final sentence: we need to protect nature "if participants are to derive enjoyment and satisfaction from their recreation experiences, now and in the future, whatever that may be."[45]

---

[43] Bocking (2000), x.

[44] Butler (1989), 309. Ecologists also were prone to this shift in language. Watt noted that one reason for using computer simulations was to prevent the effects trial and error experiments might have on ecosystems. These effects "might have a ruinous effect on the system being experimented with, which would have a prohibitive cost" (Watt 1966a, 5).

Finally, we can note that OR's emphasis on recreational impacts in general and minimum impact practices in particular actually may not reflect important ecological impacts, after all. The concerns that OR expresses are often less ecologically important than they are socially important. Certainly, there is an ecological impact in sleeping on top of flowers and grasses, but not to the same degree as the impacts of logging or mining, which minimum impact standards do not address at all. In fact, many studies on recreational impacts found that matted-down grass rebounds quite quickly and even the more noticeable impacts, such as the crushing of plant stems, return to their original state in a relatively short period of time.[46] The kinds of impacts that result from other activities (including global climate change and habitat fragmentation due to encroaching development) are far more serious, and recovery, if possible, will take considerably longer. This is reflected in the 1995 report by the Countryside Commission that stated that "in relation to the environmental impact of leisure activities on the English countryside, including the national parks, 'any damage to the countryside by recreation activity is heavily exceeded by industrialization, farming and urbanization and other forms of economic exploitation.'"[47] The same would no doubt be true here in Canada.

---

[45] Butler (1989), 310.
[46] For example, see Gibson et al. (2000), Hammitt and Cole (1987), and Murray (1997).
[47] Quoted in Swinnerton (1999), 210-211.

Scary 'Nature': The Use of Scare Quotes

> *Knowledge works as a tool of power.*
> *Hence it is plain that it increases with every increase of power*
> *~ Friedrich Nietzsche*[48]

Although the concepts *nature*, *natural*, *wilderness*, *pristine*, *balanced nature*, and so forth are central to both ecology and OR, they are far from simple.[49] These words are often not treated conventionally; that is, they rarely refer unproblematically to a referent. While Derrida and others have convincingly argued that all words have an excess of meaning that spills ever outward (that is, disseminates), the treatment of words like *nature* and *wilderness* in these two disciplines has an additional feature: they are commonly placed inside scare quotes. The irregular, inconsistent, but by no means rare, use of single quotation marks occurs in the earliest publications in ecology and OR and is still continuing today. Unfortunately, there is no accompanying manual on how to interpret this treatment. Conventionally, the use of scare quotes indicates sarcasm or scepticism on the part of the author. This use is similar to placing *so-called* in front of a word. In the case of ecology's and outdoor recreation's use of these terms, this is not the meaning we should be deriving from the use of scare quotes.

For example, one would not think that Campbell and Gibson were being sarcastic or dismissive when they noted that environmental groups and land owners in Illinois

---

[48] Nietzsche (1967 [1901]), 266.

[49] To avoid confusion, to the degree that it is possible, I will use these problematic terms without scare quotes even though I wish to hold them at a distance. In a very basic way, I do not understand these terms, and the uses they are put to are not very edifying in this respect. However, given their ubiquitous use in ecology and OR and the fact that I am discussing that use, it is not possible to avoid them. Essentially, then, every use of these words in my own work must be accompanied by scare quotes. However, I must distinguish my use of these concepts and the use of scare quotes by other authors where such use indicates something far less specific or clear. To make such a distinction, I reserve the use of these quotes for actual cases where the word is placed within scare quotes by the author(s). In all other instances where these words appear in my text, I will use italics, but the absence of scare quotes should not be taken to indicate an uncritical use of these terms.

"expect[ed] land managers to primarily concern themselves with the preservation of 'natural' conditions."[50] To think in such a way would be to undermine the intentions and efforts of many scientists and researchers. For example, Matthews and Knapp wrote that "stocking non-native fish may have profound effects on native biota and … the introduction of non-native fish disrupts 'naturalness' that should be an integral part of wilderness."[51] In another case, Wall and Wallis claimed that "[f]rom a predominantly 'natural' experience the camping market has become increasingly differentiated."[52] These writers are not being sarcastic; they are quite genuine and to see them otherwise is to do them a disservice. However, if these scare quotes do not indicate sarcasm or disdain, how should they be read?

Some authors have grasped certain elements of the problem. It is not uncommon to see comments acknowledging the vague nature of these concepts.[53] Mattyasovsky recognized that "the term ecological 'balance' may only be vaguely defined and does not answer the question of whether there should be a 'balanced' state at all…"[54] Mattyasovsky was aware that ecology had recently begun to question whether or not nature is balanced, but he does not comment on his use of scare quotes. I suggest that scare quotes result from the basic paradox that lies at the heart of these concepts. Mattyasovsky himself continued on to note that achieving an "'ecological balance,' or rather 'natural state,' is one of the goals of the research."[55] Although the term is vague enough to be questionable (and placed in scare quotes), Mattyasovsky still argues for its use as a research goal. Under other circumstances, the knowing use of a poorly defined term as a central concept in a research study, or, for that matter, in an entire discipline would cast serious doubt upon its validity. Here, however, the fact that scare quotes are used might speak to the awareness of authors to the excess that spills over, but they seem not know what to do with this excess.

---

[50] Campbell and Gibson (2001), 24.
[51] Matthews and Knapp (1999), 24.
[52] Wall and Wallis (1982), 351.
[53] Nelson (1978), for one, noted that the terms *wilderness*, *nature*, *protection*, and *conservation* were "too vague for management unless these general ideas can be translated into specific objectives, policies, and practices for each area under consideration," 721. Although he noted this, we should still go further and ask why these terms are so vague.
[54] Mattyasovsky (1979), 73.
[55] Mattyasovsky (1979), 73.

Upon closer examination, one sees that the two quotations of Mattyasovsky's illuminate another interesting point. In the first example, the problematic term in scare quotes was *balance*. In the second, it was *ecological balance* together. It is quite possible that Mattyasovsky did not mean anything in particular by this; nevertheless, I contend that this is another example of the text exceeding itself. What I read here is that the concept, *ecological*, does not or perhaps cannot consistently mean what it claims. In some instances it appears as a simple, conventional term, while in others it is problematic. The same can be said for *balance*. Taken together, these two instances reveal the shifting positions the words *ecological* and *balance* have in the discourse of OR and ecology. They are not meaningless; on the contrary, they overflow with meanings that cannot be pinned down. As a result, they are treated differently by both different authors and by the same author at different points in the same text.

In some cases, a strange reversal occurs. These troubling words are not always placed in scare quotes: some writers prefer to simply use them unproblematically. However, even here we can observe the influence of their paradoxical character. Consider, for example, Muir's advice in 1968 that park managers must "take hold of the natural physical and biological processes of the park and manage them, artificially if necessary, in such a manner that the known natural community can continue to function along traditional lines for all times."[56] How might the term *natural* be understood in this example? If we read it as it was written, it says that to keep an area natural we must control the natural process in it because only then can the evidence of humans be erased. If the definition of natural includes not being under human control, then the meaning becomes self-contradictory. Even when scare quotes are not present, the paradox they hint at still haunts the text. To add to this, holding an area in a static state 'for all time' does not conform to ecological understandings of how nature works. This is so even when the condition you hold it in is its natural state. Furthermore, how can one manage nature without that being artificial at some level?[57]

Other words occupy a shifting/shifty position in the discourse. Nelson suggested that early conservation efforts in Yellowstone National Park were focused on the vista, the view, and the scenery. This was, in fact, the purpose of conservation at the time: make an area as appealing to the eye as possible. "This led," he argued, "to less concern about indigenous vegetation or the protection of 'pristine' environments...."[58] Does this mean that indigenous vegetation is only 'pristine'[59] (as opposed to pristine[60])? Sometimes, the word *managed* causes problems. In 1985, Miles opened his conference presentation with the following: "[t]he problem I wish to address is that of sustaining and 'managing' for the long term one of the central resources for outdoor recreation programming—wilderness."[61] Although these quotation marks say, 'you cannot take me at face value,' at the same time they do not unambiguously point to an alternative. It would be a mistake to think this is a problem with clarity. If, for example, *pristine* is a paradoxical notion, then we cannot be clear about its referent and resolution. It will not settle down in one location; it keeps moving and shifting so that, even as it points to what it refers to, it no longer refers to it. Nor, however, is it signifying its new referent, even as it (almost) defines it.

---

[56] Quoted in Battin and Nelson (1982), 89.

[57] In a related case, on occasion, *natural* is used without scare quotes but in a way that is obviously contradictory to its definition. Carver, Evans, and Fritz (2002), for example, defined wilderness as comprising four elements: the third was apparent naturalness (which they distinguish from the fourth: biophysical naturalness). For them, apparent naturalness meant distance from nearest human artefact and the number of artefacts present inside the area. Biophysical naturalness, on the other hand, was a measure of the degree of naturalness of land cover based on intensity of human use. Although they attempted to explain the differences in the factors, paradoxes and confusion still resulted. See also Murray (1997) who used the phrase *semblance of naturalness*.

[58] Nelson (1982), 43. It is interesting that early conservation efforts were more apt to manipulate the environment than they are today. Now, the call is to leave natural areas alone as much as possible. Where this is not possible, because too much damage has already occurred, manipulation is permitted only in order to return the area to an earlier state. As Nelson observed, however, these earlier times, ironically, were times when manipulation was seen as more acceptable.

[59] In the ironic or sarcastic sense that actually means the opposite.

[60] In the more straightforward usage where it means what it says—whatever that may be.

If the word *natural* is problematic, it makes sense to assume that its opposite would be, as well. In 1987, Hammitt and Cole illustrated this when they encouraged managers not to be "paralyzed by a concern with avoidance of engineering if it is the only means of avoiding equally 'unnatural' resource damage [from overuse]."[62] If we read the scare quotes as indicative of an inversion of the meaning of the word *unnatural*, does this mean it is not unnatural to destroy natural resources through overuse? If both engineering and overuse result in unnatural changes, why do these authors try to reject overuse on the grounds that engineering is better because it is more natural? Later in their text, they posited that covering areas such as campsites with a durable surface would help reduce the damaging effects of backcountry use. This would be a drastic step that would interfere with the naturalness of the area. However, they suggested that "it is debatable whether surfaced areas are any less 'natural' than barren, dusty or muddy, devegetated areas."[63] So, artificial covering is as natural as a campsite flattened by hikers? Is it more natural? Less natural? What about bison wallows or other naturally ('naturally'?) barren areas? These are examples of the text saying more than it means to say, more than it wants, more than it can contain. Hammitt and Cole's examples are particularly significant when one remembers that their book, *Wildland Recreation: Ecology and Management*, is the single most comprehensive work on ecological impacts and recreation ever written.[64]

---

[61] Miles (1985), 95.

[62] Hammitt and Cole (1987), 285.

Related to the question of unnatural is the problem of constructed natural environments. If many of our wilderness areas are now so impacted that leaving them alone will not guarantee their return to a natural state, many researchers have advocated for an active restoration of natural conditions. In these texts, the problematic nature of the word *nature* is even more acute. It flies in the face of the very definition of natural to have to create it artificially. On the other hand, it is also obvious that leaving these areas alone will not produce the desired conditions. In an article that discussed backcountry user preferences for tree harvesting techniques, Cook and Walter concluded that "[t]here is no single prescription suitable for all situations, but it seems clear that a *combination* of cutting methods would produce a more 'natural' mixture of species and sizes than would any one type of harvest cut."[65] The suggestion here is that, because users prefer various types of trees, a variety of cut methods would produce such an effect, which in turn would make it look more appealing. What Cook and Walter do not state directly is whether or not the preferred conditions are natural or simply preferred. By placing

---

[63] Hammitt and Cole (1987), 302.

[64] See also Battin and Nelson (1982) who claimed that "[b]etween 1949 and 1953 several 'improvements' were made to park facilities," 79. Engineering in parks seems to present persistent problems for OR scholars.

[65] Cook Jr. (1972), 301.

natural in scare quotes, they suggest as much, but, at the same time, they stop short of actually stating it. This also does not take into account the studies that suggest that user preferences are not always in keeping with indicators of the ecological health of an area.

Outdoor recreation researchers are not the only ones who encounter these difficulties. From the beginning, ecologists have also employed the use of scare quotes in a similarly confusing fashion. For example, in Chapter Two, I quoted Nichols' claim that a classification system "should be 'natural' to the extent that it is based on principles and relationships which exist in nature."[66] More recently, as ecology began investigating influences of radiation on ecosystems, Philip Gustafson had this to say in terms of research benefits: the "fallout has permitted other aspects of 'the natural environment' to be investigated by novel and creative means, often along new and unique lines."[67] Ecologists have encountered this problem from the very beginnings of their field. Tansley, for example, criticized Clements' understanding of formation and climax because it neglected the great number of instances where deviations occurred, for example, "the far-reaching 'telescoping' of phases, the appearance of plants or populations out of their 'proper' order and so on."[68] Tansley did not mean that there was no order to plant or population changes; he still supported the idea of succession, but it is not clear whether he used the word *proper* to mean that his view was more natural than Clements'.

---

[66] Nichols (1923b), 155.

[67] Quoted in Golley (1993), 73. Studies of radiation or chemical release often use scare quotes. Possibly, this use is one instance that reflects sarcasm or irony. Lands that have been irradiated or sprayed with chemicals, I suggest, can only be called natural in an ironic sense. However, even if Gustafson was being ironic, he did not comment on this and left the reader to interpret the meaning of his use of scare quotes.

[68] A. G. T. (1916), 201.

Words other than *natural* caused problems for ecologists. Consider Hickie, who, in 1957, contended that "[a] primary purpose of wildlife management is to have enough animals (birds and mammals) of the 'right' kinds—and conversely, not too many of the 'wrong' kinds—in a given area at a given time."[69] Similarly, Battin and Nelson noted that in the early years of Canada's national park system "the protection of certain 'desirable' animals such as elk and deer was encouraged."[70] As with the above examples, these authors should not be read sarcastically. Even with Battin and Nelson, it is too simple to say they were only indicating that early views on wildlife conservation are now outdated. Quite probably they do want to distance themselves from this early position that privileged certain animals over others for aesthetic and economic reasons, but at the same time, most ecologists and conservationists do see certain species as more desirable than others. For example, it is common among both OR researchers and ecologists to describe species that do not belong as exotic or invading. For plant species, these are often called weeds or noxious weeds.[71] For animal or insect ecologists, the term of preference is pest.[72]

One longstanding topic of ecological research bears directly on questions of indigenous species. Starting with the first few issues of *The Journal of Ecology*, the debate around edaphism or endemism increased and continued up until at least the late 1960s.[73] This debate attempted to answer the question, 'What were the original inhabitants of an area?' Once this was answered, all other species could be labelled invaders or exotics. These latecomers were thought not to belong; they were unwanted and even unnatural in that location. This long debate in ecology is similar to the use of scare quotes because the difficulty in many instances did not lie in determining what species came into the area when. The problem lay in the very concept of edaphism or endemism. How long could a species be present before it was considered indigenous? Did the method of arrival have any impact on a species' status? For example, if it was discovered that humans (or pack animals) transported this species, must it always be an invader? Fundamentally, these questions and uncertainties arose from the more basic question of what constitutes a natural environment. It is paradoxical to read the debates about endemism in a straightforward manner, that is, without scare quotes. On the other hand, it is no clearer to read them with scare quotes.

---

[69] Hickie (1957), 55.
[70] Battin and Nelson (1982), 86.
[71] For example, Hendee and Dawson (2001), Hendee and Dawson (2004), McGivney (1998), Mentzer (1951), Noss (1996), and Selleck, Coupland, and Frankton (1962).
[72] For example, Hoffmann et al. (1949), Lister and Kay (2000), Marsh (1970), Morton (2000), Odum (1983), Pimentel (1966), and Watt (1966b).
[73] See A. G. T. (1914 and 1916), The interpretation and application of certain terms and concepts in the ecological classification of plant communities (1919), Meyer (1937), Cain (1947), Daubenmire (1952), and Stebbins and Major (1965).

Tansley offers one more twist on the use of the concept *natural*. He argued that ecologists should develop a system of ecological concepts that included all forms of vegetation. "We cannot," he continued, "confine ourselves to the so-called 'natural' entities and ignore the processes and expressions of vegetation now so abundantly provided us by the activities of man … because scientific analysis must penetrate beneath the forms of the 'natural' entities."[74] Here Tansley uses *so-called* and scare quotes to modify the meaning of *natural*. If both scare quotes and *so-called* are used to invert the meaning of a word, does this double inversion return the concept natural back to meaning nonhuman or without any trace of human influence? What would be the difference, then, between "the so-called 'natural' entities," "the 'natural' entities," "the so-called natural entities" or "the natural entities"? That Tansley felt the need to subject this term to single, or what amounts to double, scare quotes without an accompanying explanation, illustrates two points: (1) Certain words are not treated as simple signs with a clear meaning. These words somehow need to be marked off and separated from the rest of the text. (2) At the same time, there is an inability to provide a cipher to guide readers in making sense of these words. In fact, if one reads scare quotes in a conventional manner, the meaning becomes circular and paradoxical.

---

[74] Tansley (1935), 304.

No matter how the concept *natural* (as well as the many other related terms) is used, it seems unsatisfactory; yet, it also seems equally essential to both ecology and OR. Scare quotes are one attempt at addressing this problem. The use of these textual tactics alerts us to the problematic character of these terms. From there we can ask why these terms are treated this way and whether another set of terms might be clearer. In the end, scare quotes are of limited use because they mainly postpone a close examination of the issues. Certainly, this is understandable and even commendable when ecology and OR are often concerned to protect the dwindling wilderness areas in Canada. If ecologists and OR scholars were sidetracked in a debate over the slipperiness of certain concepts, preservation efforts might have suffered. Nonetheless, I argue that being clearer about the meaning of pivotal terms would actually strengthen preservation efforts because the ambiguity inherent in scare quotes would be removed.

Paradoxes

> *Nature is an infinite sphere of which the center*
> *is everywhere and the circumference nowhere.*
> *~ Pascal, 'Pensées'*

Natural, *wild*, *wilderness*, and *nature* are some of the key concepts in outdoor recreation and ecology. Although it is difficult to argue that they are not central to ecology and outdoor recreation, it is far more difficult to decide what they mean.[75] My aim here is not to suggest that these concepts are without meaning (indeed, I think they overflow with meanings they cannot contain), nor is it to suggest they are unimportant—I fully realise the importance these concepts have had in the preservation and protection of nonhuman life and life systems. The gravity of this issue is attested in ***Assault on the Rockies:*** *Environmental Controversies in Alberta,* in which Ian Urquhart calls attention to the fact that the energy and forestry industries "have left Alberta's Foothills without virtually any wilderness all—0.39 per cent."[76] Rather than undermine the importance of natural areas, wilderness zones, scientific reserves, and other types of wilderness areas, I aim to determine what words such as *wilderness*, *natural*, *pristine*, and *wild* have meant to different scholars.

The basic commonality of these concepts lies in distinguishing between what is and what is not human. Regardless of how one attempts to make and maintain this division, there is an excess that spills over the text, an excess that threatens to undo the text. This excess often appears as a paradox, and the problem begins the moment we try to explain or define these concepts. Carter noted this paradox when he stated that

> managing wilderness (designated or not) let alone wildlife in wilderness, is a paradox. Such a concept implies forcing a modern and planned human system into a chaotic and untrammeled wild place. Controlling wilderness (wildness) is the purpose of management, precisely the arrogance that destroys wilderness (wildness).[77]

---

[75] See Swinnerton (1999) who commented "[t]hat outdoor recreation and visitor facilities often put stress on the environment … [which] raises the issue as to what constitutes a risk to nature and natural systems if such areas are not entirely natural in the first place" 202.

[76] Urquhart (1998), 3.

[77] Carter (1997), 17.

Chaotic and *untrammelled* are often used as distinguishing features of wilderness and wildness. They conjure images of animals and processes running wild and free without the influence of humans. Thus, Carter said, it is paradoxical for humans to control or manage it.

In his 1976 book on outdoor recreation planning, Jubeville stated that "[t]he key to understanding naturalism and aesthetics is based not on whether the landscape is pristine but whether the landscape conditions (including man-made developments) are appropriate for the particular experience." In support of the claim that pristine conditions are not key he cites the controversy over the Bitterroot Mountains: "[t]he large symmetrical blocks were not natural and consequently reduced the natural aesthetic appeal of the mountain range. Smaller, asymmetrical openings would have coincided more with natural conditions."[78] Here again the text cannot be made to say what it wants. Jubenville begins with the statement that pristine conditions are not central but ends with an example that appears to regard them as a key factor in determining the value of an area. The concept of *pristine* is troubling. Moreover, the separation of pristine nature from culture/humans embroils one in a paradox. Here is the inherent excess that deconstructs the text.

Ted Mosquin, while speaking of biodiversity, exemplified another troubling aspect of the concept of *natural*. He remarked that "biological, ecological, and medical science have enabled our species to escape the laws governing the stability of planetary norms. Considering that the earth and its resources are finite, this escape, logically, is temporary."[79] If we have escaped planetary norms, to where have we escaped? When nature is defined as non-human, and natural systems are planetary, if we are not within nature, we would have no place to be. This is another form of the paradox of the human/nature split. There is no doubt that humanity has had significant impacts the world over; however, I remain unconvinced that it is the best option to call this impact *unnatural*.

---

[78] Jubenville (1976), 65-66.
[79] Mosquin (2000), 60.

In another example of the murky definition of these terms, Holling stated that the increasing consequences of human actions in the world have necessitated changes in "both science and management of natural and managed environments…."[80] Management of managed environments may be redundant phrasing, but it makes sense. However, the concept of *natural* performs an odd and shifting operation here. Both Carter and Holling claimed to be able to manage natural places and wilderness without turning them into managed areas. How is a managed natural area different from a managed managed one? The term nature occupies a dangerous zone of liminality, undecidability, and indeterminacy. Its use seems to be paradoxical no matter how one treats it, and it is always possible to read other meanings from its use. The use of scare quotes may be one attempt at blocking these alternative readings or recognizing the inherent paradoxes; however, adding scare quotes actually adds another layer to be interpreted.

If the concept of managing natural areas proves to be paradoxical, then perhaps we should simply leave them alone and let nature take its course. Unfortunately, even the idea of leaving nature alone to do its own thing is more complicated than might appear at first. Cole recently remarked that

> [s]everal decades ago managers and policymakers assumed that natural conditions could be perpetuated by leaving nature alone. Today, this assumption is untenable. Contemporary human activities and influences

---

[80] Holling (1992), 484.

> (fire suppression, climate change, and much more) are altering conditions in all wilderness areas. Some wilderness managers have adopted restoration programs to compensate for this loss. Yet restoration, despite its admirable intent, is a form of control over wilderness conditions.[81]

It seems that nature, if left alone, will not be natural. Our influence is so profound and pervasive that even nature is no longer natural. Of course, to make nature natural would be to interfere; thus, we are back to the problem identified above of the paradox of managing wilderness and nature. The idea that nature can exist without human impacts and free from human control has embedded within it a paradox that does not seem resolvable. This paradox runs deeper than just our understandings of what backcountry wilderness is; it is a paradox that touches the core of the meanings that Western culture has, since the Enlightenment, ascribed to humanity.

In the 1960s, around the time when The Wilderness Act was passed into law in the US, Cole has argued, there was little understanding that naturalness could be lost if it was not intentionally manipulated. He stated further that wilderness was originally intended to designate areas off-limits to human engineering. Now, ironically, we are having to reverse this designation. Due to the level of impacts recreation has had on wilderness areas, engineering is now seen by some scholars as necessary to return them to a natural state. However, "[i]f we do so, the wildness of wilderness will be diminished."[82] For Cole, leaving wilderness alone may make it wild, but it will not keep it natural. Naturalness can only be achieved nowadays through manipulation. On the other hand, leaving wilderness wild through a hands-off approach allows human influence to spread through it. In the end, both wildness and naturalness result in or require human presence. Of the four concepts in Cole's title (*natural*, *wild*, *uncrowded*, and *free*), the only one that relates to the absence of humans is uncrowded. Yet, this uncrowded state does not simply occur; it must be manipulated or engineered. It is an enforced and constructed absence: trails twist and turn, obscuring lines of sight to other users, user limits are set, campsites are placed apart from each other and arranged so as to make it more difficult to see or hear others, and backcountry users are encouraged to use neutral colours that blend in with their surroundings.

---

[81] Cole (2000), 5.
[82] Cole (2000), 6.

Cole was not the first person to wrestle with this problem. Over twenty years earlier, Reid promoted the suggestion that there are two approaches to nature preservation in Canada. The first refers

> to the preservation of the natural ecological process of some defined area, that is, permitting the vegetation, soils, wildlife, and so on, of the area to proceed freely along their natural ecological courses without the deliberate intervention of man.... The second concept of nature preservation is a more populist approach, in which some particular segment of an ecosystem, or some particular seral stage, is preserved because of its immediate social desirability.[83]

The second approach might require manipulation while the first prohibits it, although, as Reid admitted, there are exceptions in the first concept; for example, wild fires may have to be contained or suppressed in wilderness areas. Furthermore, "[r]elatively few Canadian ecosystems have not been man-modified, and deliberate manipulation may be required to recreate the natural sequence."[84] So, according to this definition, wilderness areas cannot bear marks of deliberate human alteration. This prohibition against alteration is true for all wilderness areas except where alterations are needed to remove the signs of previous alterations that were not natural because they were deliberate and therefore not classed in the first set of natural alterations (except, of course, for those exceptions to the first set of alterations). As one can see, the concept of natural areas is far from simple or clear.

---

[83] Reid (1978), 106.
[84] Reid (1978), 107.

Even before Reid, Cowan encountered this paradox while discussing the role ecologists play in the development of natural area planning and management. He noted that the ecologist could provide input into trail, campsite, and facility design and location. Trails need to be carefully planned, he contended, because we want to maximize the recreational experience while minimizing the ecological impact. Thus, Cowan suggested that breeding areas

> will be skirted, salt licks and bedding grounds will be approached from the appropriate side, choice groves of trees will be left intact, ecological areas reserved for study will be by-passed entirely, fills and excavations will be kept to a minimum, hills and curves are a feature of the landscape to be enjoyed and lived with in sensitivity—not seen as a challenge to the bulldozer. Straight stretches of road are to be avoided like the plague, unless they serve the recreational objective, they invite speed and boredom. Where lake shores or river banks are approached the objective becomes minimum disturbance for maximum artistic exposure of scenery and ecological diversity. The peak or glacier framed by trees, the beaver pond, the pondweed bed where moose will be seen morning and evening, exposed just enough and from the best direction for viewing and photography.[85]

The level of manipulation this passage encouraged is significant; yet, Cowan still contended that this breeding area was natural (by which he meant no human influence). In this construction, nature and human manipulation co-exist in harmony. This example dates from 1970; however, there are other, more prevalent and more recent, examples of the inherent paradox between the natural and the social.

Consider the Leave No Trace! principles that were adopted in the early 1980s. By the 1990s they were standard across North America in one guise or another. Although LNT was started as a means to protect wilderness areas from excessive ecological impact, an unacknowledged component was also present from the beginning. Consistently, one of the tenets of LNT has been to be considerate of other visitors. Consideration meant a number of things in different settings. It might mean, for example, that hikers should move off the trail in order to permit others to pass. It might mean that groups should not make too much noise. It could also mean that people should use neutral colours for clothing, tents, and packs. Garbage and other obvious signs of use were discouraged not only for ecological but also for social reasons.[86] This mixing of social and natural factors in the LNT program is not a problem. I want to draw attention to the fact that conflation of social and natural factors happens while at the same time the discourse claims LNT deals solely with reducing ecological impacts.[87] Maintaining solitude and being courteous of others is certainly important. What is interesting is that this element seems to slide in unacknowledged.

---

[85] Cowan (1970), 324.

In 1989, one of the foremost Canadian researchers on wilderness recreation impacts attempted to define environment, natural environment, and artificial environment. This is a commendable and far too uncommon effort that also allows us to see how paradoxes creep into OR literature. According to Wall, the environment is the sum total of conditions that surround us. Dividing this category into two, he argued that the natural environment "refers to nature, more specifically aspects of nature which are natural in that they have not been modified by human beings."[88] Lastly, he suggested "[n]atural environments are contrasted with man-modified environments, such as urban environments, which are often called built environments."[89] This is an excellent example of the circularity one necessarily encounters when defining natural nature. In definitions of *nature* or *natural* neither word can be used in the definition. Wall's discussion of the concept of artificial offers little by way of clarification: artificial areas are those in contrast to natural ones. Although complications abound, Wall was able to continue opposing natural environment with artificial environment throughout his chapter without resolving them.

---

[86] See United States Department of Agriculture Forest Service (1994) and National Outdoor Leadership School (1995).

[87] In the NOLS booklet, six principles for LNT are listed, none of which addresses the social element. In the back of the booklet, impact is defined in such a way that human impacts are included but are not regarded as being as significant as the ecological ones. Yet, the social factor is the topic of the first page of the booklet, as if it were the main reason for LNT. For example, the booklet stated that people seek wilderness "to get away from the crowds, noise and daily pressures of urban life" (National Outdoor Leadership School 1995, 2). As a result, people should first reduce the evidence of their visit and then minimize the disturbance to the local ecology. The exam at the end also contains more questions about social impacts than about ecological ones. That the text of the booklet consistently treats the social ahead of the ecological but defines the goal of LNT as primarily to maintain ecological health is revealing.

These paradoxes in OR are, in some ways, offshoots of the paradoxes ecologists have been dealing with since the early 1900s. During the 1960s, the ecologist Niering studied atoll ecosystems in the Pacific Ocean. His work is illustrative of one consequence that can arise when we try to maintain the split between humans and nature. Niering stated that "[a]s one views man as part of the atoll ecosystem, it is strikingly evident that he is dependent upon both the terrestrial and marine environments for his survival."[90] This is interesting because it suggests that the local people there cannot be separated from the natural systems upon which they depend. Further on, Niering directly placed humans within the framework of the atoll ecosystem: "[t]he atoll is an essentially self-sufficient microcosm in which man is a key component in balance with his environment."[91] However, this position is not consistently maintained throughout his article. On the same page where he located humans as a key component of the ecosystem, Niering noted that

---

[88] Wall (1989), 204.
[89] Wall (1989), 204.
[90] Niering (1963), 156.
[91] Niering (1963), 158.

"[w]ithin the plantations, the natural vegetation is restricted to the undergrowth."[92] On plantations, in other words, the results of people's agricultural efforts restricted natural vegetation to the undergrowth. Niering did not list this vegetation by species; instead, he used the broader term *natural* to indicate its distinction from the agricultural vegetation planted by the local people. Niering's treatment of vegetation on the plantation from vegetation off the plantation makes for a confusing distinction given his earlier argument that humans are an integral part of the natural environment. How can an integral part of this environment be unnatural? Does this mean that some aspects of the natural environment are not natural when they are controlled by humans (who somehow still remain natural)? Or perhaps the environment itself is not so natural, after all? In which case, one would have to admit that the vegetation not planted by the local people is not part of the natural environment precisely because it is natural. The inconsistency noted here does not stem from a failing on Niering's part. It has its roots further back in the Enlightenment and is much more pervasive than any single researcher's discussion. The very idea that humans can be extricated from natural systems is paradoxical, and the only way to maintain this distinction is to avoid discussing it and the inevitable inconsistencies that arise when one employs it.

As I remarked above, throughout the twentieth century certain species have been labelled undesirable. These are regularly called invader species. The concept of an invader presupposes that it does not belong. Many ecological and OR studies have been conducted on invading plants, trees, shrubs, insects, and animals. In nearly all cases, researchers assert that invasions of exotic species into natural areas delay the arrival of the climax state. In addition to these types of invaders, natural disasters can also upset the successional pattern of an area. Fire is one of the more striking examples of this phenomenon. In some ways, when a fire moves through an area it is akin to resetting the successional clock (although not always, as colonizers in these secondary successions are not always the same as the original ones and the whole trajectory of succession can be altered).[93] Regardless of the specific type of invader, all of them can threaten the natural pattern of succession to one degree or another.[94]

---

[92] Niering (1963), 158.

In Coupland's 1950 study of the mixed prairie in Canada, one can see the paradox associated with the idea of invaders. He stated that invaders enter the grassland wherever the cover has been disturbed. "The principal agents of disturbance," he claimed, "are animals and man." [95] It may be easy to understand how the action of farmers on the soil could be considered disruptive to the natural pattern of succession in the prairie, but the case of animals is not as clear. It appears, because gophers dig up soil and pile it in mounds that cover the climax grasses, that gophers disrupt the natural successional pattern. If these animals disrupt the natural process, then they are not part of the natural system of the prairie. If they do not belong where they live, where do they belong?

This is not the only way to perceive the action of gophers. By 1994, Wu and Levin were regarding them quite differently.[96] They concluded that the gophers, by destroying individual plants and creating patchiness in the grassland landscape, mark but another method by which the ecosystem changes. Many forces create patchiness in the landscape. Stochastic (chance) events create different patterns of change than the earlier models of succession allowed. Heterogeneity marks these process and their results. Because of the altered perspective this model requires, gopher activity becomes part of the understanding of how a grassland ecosystem functions. Wu and Levin appear to have solved the gopher paradox. This is one of the most promising aspects of newer ecological models and will be discussed more fully in Chapter Eight.

---

[93] See Bard (1952), Benedetti-Cecchi (2000), and Keever (1950).

[94] This is distinct from Clements' early formulations of his model of succession where the former stage makes conditions unfavourable for itself but favourable for the invaders of the next phase. This aspect of the model had been more or less successfully challenged by the 1950s. Thereafter, invaders were not seen as part of the normal progression to a climax state, but, rather, as unpredictable and/or disruptive events that potentially could postpone the climax state indefinitely.

[95] Coupland (1950), 310.

[96] Wu and Levin (1994), 461.

Only two years before Wu and Levin, Holling discussed the average body-mass categories for various species, and drafted tables depicting the body-mass clumps for them. His study is another example of the effects of the human/nature paradox. He stated that humans are an ecological animal unique in terms of body-mass clump categories: our body-mass clump category "lies in the gap between the second largest body-mass clump category for mammals and the largest one."[97] Specifically, our average body-mass falls between the wolf (number two) and the deer (number one). However, there is no number between one and two, so how can we fall there? I suggest it is because Holling sees the human body-mass clump as an importation onto the natural body-mass clumps that animals fall into: If we were to interject humans into this natural scale, he might have said, we would find that they fall directly between the deer and the wolf. This is another example of the paradoxical position humans occupy in ecology: sometimes we are an ecological animal and other times we are most certainly not. In the first sense, we are ecological in a sort of vacuous generalization (for example, 'humans are part of the ecosystem'). In Holling's configuration we exist in a nowhere land that is neither the first category nor the second one but somewhere in between them (the first-and-a-half category?), not as an ecological animal but as a mammal nonetheless. I submit that the role humans play in this model, a role that admits that our actions can threaten nature and at the same time views these actions as necessary for nature, positions humans as indeterminate, undecidable, or liminal. This is a paradoxical position that is neither in nature nor out of it. In this liminal state there are no clear rules or guideposts.

---

[97] Holling (1992), 482.

Concepts such as *wildness*, *wilderness*, *nature*, *natural,* and *pristine* all invoke more meaning than they can contain. Each term requires that which it is not compatible with in order to function. Try as they might, neither ecologists nor OR scholars could avoid encountering this dilemma regularly in their writing. I contend that this is not due to the poor use of logic or sloppiness on their part; the problem is located in the very paradoxical character of these concepts. This is a characteristic that cannot be excised. Paradoxes can, however, appear without explicit treatment. Both the discourse of ecology and OR contain these paradoxes and challenges, but rarely have they theorized them. As Worster noted, "few scientists have perceived people or human societies as being integral parts of their ecosystems. They leave them out as distractions, imponderables."[98] In spite of their efforts, ecologists and OR researchers have not been entirely successful in their task of separating humans from nature. The influence of humans, it seems, is coming back to haunt nature, reminding us of its presence, demanding that it be taken into account. This haunting will be explored in detail in Chapter Five and helps us understand how it is that elements thought to be removed can return to challenge our modelling and theorizing.

---

[98] Worster (1995), 19 and 24.

CHAPTER 5: TRACING AND HAUNTING HUMANS AND/IN NATURE

> *Facts are simple and facts are straight*
> *Facts are lazy and facts are late*
> *Facts all come with points of view*
> *Facts don't do what I want them to*
> *Facts just twist the truth around*
> *Facts are living turned inside out*
> *Facts are getting the best of them*
> *Facts are nothing on the face of things*
> ~ *The Talking Heads, 'Cross-Eyed and Painless'*

This chapter examines the ways in which the human presence returns to haunt both ecological and OR discourse even as most scholars explicitly reject this figure. I am suggesting that both these discourses make overt attempts to exclude the human element in their treatment of natural nature. Furthermore, physical spaces are expressly built and managed with the intention to reduce or if possible to eliminate all signs and effects of human beings. In OR, a great deal of time and energy has been expended learning how best to communicate and ensure compliance with minimum impact practices. These practices revolve around the idea that people are not part of wilderness and, when they travel through it, they should try to remove all traces of their passing. Nonetheless, there are excesses that continually slide over and out of the frameworks ecological and OR discourse provide. These excesses result in a type of necessary failure in science in general and in ecology in particular. Since OR claims a connection with science and ecology, these necessary failures are incorporated as traces and hauntings in OR discourse, as well.

In Chapter Two I argued that science has a particular way of interacting with the world that is characterized by a will to know. The will to knowledge is not unique to ecology nor did it begin with it.

> [g]oing back a little in time, to the turn of the sixteenth and seventeenth centuries—and particularly in England—a will to knowledge emerged which, anticipating its present content, sketched out a schema of possible, observable, measurable and classifiable objects; a will to knowledge which imposed upon the knowing subject—in some ways taking precedence over all experience—a certain position, a certain viewpoint, and a certain function (look rather than read, verify rather than comment), a will to knowledge which prescribed (and, more generally speaking, all instruments determined) the technological level at which knowledge could be employed in order to be verifiable and useful.[1]

Foucault's suggestion that the will to know made possible the measurement of objects does not cast doubt on whether this will to knowledge did in fact lead to verifiable, useful, and universal laws. The switch from older forms of knowledge to scientific thinking represented a profound change in (1) the structure of knowledge, (2) the positions from which knowledgeable people spoke authoritatively about the world, (3) and the practices of science; however, validating objectivity over subjectivity or vice versa is not the concern here.

What is of interest here is the advent of the search itself (for definitive, objective, and often universalized answers), rather than questions of validity. How, exactly, was the modern disciplinary regime implicated in this search, and what were the effects and limitations on ecological knowledge and outdoor recreation research and practice? These questions form the background against which Chapters Two and Three should be read. This present chapter shows the implications of how science was conducted and the rules for generating statements in ecology and OR. More specifically, I illustrate not how the discursive practices in ecology and OR resulted in the removal of humans from natural nature but, rather, how they produced a certain spectral figure that returned again and again to haunt the very foundation of both discourses. To begin, however, I examine instances where ecologists were searching for universal categories and predictive models and encountered challenges or threats to the very structure of scientific investigation. Ironically, it was by following the scientific method itself that ecologists encountered these threats.

---

[1] Foucault (1972b), 218.

The Necessary Failures of Normative Ecology

Probably the most visible example of unintended consequences is what happens every time humans try to change the natural ecology of a place.
~ Margaret J. Wheatley

As noted in Chapter Two, one of the main thrusts of ecological investigation was the production of knowledge about nature that was certain and true. This thrust meant, in part, that definitions needed to be universal and accurate, experiments conducted objectively, and knowledge fitted into a progressive and developmental structure. Although this was the intention, it proved to be much more difficult to achieve. Early on in the development of ecology, difficulties came to the attention of ecologists. It is a testament to the power of the rules of formation and the modalities of enunciation that governed ecological discourse at the time that ecologists continually saw these difficulties as further proof of the progressive nature of science. Ecologists, in attempts to respond to these difficulties, questioned each other, repudiated each other, sometimes rebuked themselves, and in rare cases ridiculed each other, but in no instances did they question the basic design of science, the assumptions it required, or the rules that governed it. The archive from which ecology drafted its statements had remarkably fixed the production rules for ecological statements. When this fixity squared off against instances of flux and uncertainty, the ecological knowledge produced was in keeping with the fixed archival system of the disciplinary regime. Thus, moments of flux and indeterminacy were converted into normative statements, models, concepts, and theories.

One of the earliest instances of the difficulties that arose by following the scientific method occurred in the *Journal of Ecology.* In an anonymous review of Nichols' book on plant classification the reviewer discussed the term 'association' and noted that Nichols saw it as "a group or community of plants occupying a common habitat, which again he defines as a unit area with an essentially uniform environment."[2] However, the reviewer argued, it was very hard to determine what constituted an 'essentially uniform' environment. In fact, the reviewer observed, it was impossible. The reviewer claimed that it did not matter what definition was used to explain associations, "[w]e are impressing our concepts [of association and species] on nature … but they are good workable concepts for all that, because there is, ordinarily, a certain discontinuity between the types of well-marked associations as between the types of well-marked species."[3] The potential of the first portion of this remark to engage humans more intimately with natural nature, unfortunately, was retracted by the second half. Admitting that ecologists impress concepts on nature implies that whatever universality was found was more an exercise of power than a simple reflection of a law of nature, which certainly is a bold claim for 1919. However, the second half of this comment steers clear of the more radical implications of this claim. If no one can really know what an association is, and if we always impress our concepts onto nature out of necessity, does this not bring into question our understanding of factual knowledge? Ecology charges itself with understanding nature as it is 'out there' without the taint of human bias. Given the first part of what this reviewer said, it seemed ecology was doomed to fail its assignment. However, the discomfort this realisation could provoke was avoided by the turn back to a more normative understanding of a nature divided into clear-cut associations.

---

[2] The interpretation and application of certain terms and concepts in the ecological classification of plant communities (1919), 101.

[3] The interpretation and application of certain terms and concepts in the ecological classification of plant communities (1919), 101.

For science to have the reach and influence it did, the terms and models used needed to be accurate and consistent. However important this requirement was, failure to meet it did not undermine the whole scientific process. It may be that associations are categorically impossible to define; it may be that ecologists impress their concepts onto nature instead of dispassionately looking at it, but the implications of this were not recognized. This reviewer came close to questioning the structure of scientific knowledge, probably without realizing it. In any case, the threat that these observations could have produced is further nullified a few lines later when he said, "[b]roadly speaking we all know an association when we see one, or rather when we have had some experience of it and its neighbours."[4] Although we cannot clearly, consistently, or accurately define associations, although we know we have imposed them onto nature instead of simply finding them there, we are reassured in the end because they are obviously still 'out there,' all we need do is look and see them and their neighbours all around us.

At a broader level, the very idea of a natural area was proving difficult for ecology. In a 1947 symposium discussing natural floristic areas in North America, Cain conceded that "it is no simple problem to ascertain what is a natural area."[5] He continued to say that when natural areas are mapped, "it becomes impossible to draw boundaries with any degree of accuracy except for the smallest natural areas when mapped on a very large scale."[6] One would be tempted to think, based on these comments, that natural areas are actually difficult to define, and yet two pages later Cain will remark that even in the beginnings of floristic characterization of geographic regions in the mid-1700s "[i]t soon became apparent that plants are not indiscriminately distributed, that each species has a definable area, that there are pattern types for the areas of different species"[7] Somewhere between the definitive distribution of plant species and the boundaries that comprise a natural area, uncertainty creeps in about the reality of natural areas.

---

[4] The interpretation and application of certain terms and concepts in the ecological classification of plant communities (1919), 101.
[5] Cain (1947), 187.

Cain called the pattern types that plant species displayed the association type.[8] Interestingly, although Cain said that from the beginning of the 1900s to at least 1940 there had been "the tacit assumption that the association is floristically characterized and has objective reality," he also noted that "geobotanists are somewhat bothered by the fact that the floristic assemblages of certain, often numerous, stands are not typical of any recognized plant association, nor simply transitional between two associations."[9] In other words, these constituent species were in a liminal state; they did not clearly belong to any one association nor were they in transition between two. For example, Cain observed that various stands of the beech-maple association (as defined by others) contain such a variety of species, dominants, and co-dominants that it begs the question of whether it should be thought of as a single entity (the beech-maple association type) or whether each stand should be called an association individual with strictly limited distribution. In the end, Cain took the position that associations are objective, but not in either the large or small sense others have claimed. He concluded that "[t]he association concept … is a strictly local phenomenon and even then not subject to severe definition."[10] Those that sought to prove the objective reality of associations beyond this, he argued, produce great studies with large statistical data sets. However, "there is more artifice here than science in the selection of stands for representation of the association."[11] In other words, associations became real when ecologists artificially manipulated the variables until they got the desired result. If ecologists ceased this manipulation, they would see that associations are really not real.

---

[6] Cain (1947), 188.
[7] Cain (1947), 189.
[8] As with most terms in ecology, association type was not universally recognized nor consistently used. Cain used it to refer to what he saw as the predictable patterns that characterized a certain association type. For example, jack pines might form an association in one location. If there was a pattern to this association, we should see it mirrored in nearly all jack pine associations in North American (or in similar climatic regions). This consistent pattern was called the association type.
[9] Cain (1947), 194. Note the down-playing of the issue here. Ecologists were only somewhat bothered, as if it was not worth being too concerned over.

This confusing vacillation, which runs through the works Cain reviewed, between seeing the association and association type as definite objects and seeing them as complex, contested, and without any objective reality was typical of the field for the first half of the twentieth century. Cain ended by defining a natural area as somewhat indefinite. The indefiniteness of natural areas, he suggested, should be seen as one of its main strengths, not a weakness. This is another indication of scientists grappling with the problems their discipline threw up in front of them, problems that the scientific method led directly to and that threatened some of the foundations of that very method. In an odd catch-22, the better the scientific method worked, the more it cast doubt on itself. As Cain noted, the most useful understanding of associations that ecology could deliver was also the least well-defined and demarcated.

---

[10] Cain (1947), 195.
[11] Cain (1947), 197.

The problem of boundaries that Cain faced was repeated nearly twenty years later in Cooper's study of the dynamics of aquatic animal populations. The population Cooper dealt with had clearly delineated dynamics and a determined size. Furthermore, "[t]he annual pattern of population size and structure," he argued, "is probably representative of most aquatic, invertebrate populations."[12] Following this, however, Cooper listed a number of studies that contradicted his generalization and showed that determining population size and dynamics are not as simple as his study indicated. Ecologists were struggling, along with Cain, to find consistent measurements for the basic units they were discussing.

One of the basic requirements of science is to have consistently defined and clearly marked units of study. These ecologists certainly sought this type of consistency. It seemed, however, to elude them. Eugene Odum, one of the most influential ecologists of the last half of the 1900s, wanted to unify ecology by developing a single, overarching concept that all studies could use. In the 1970s, he turned to the ecosystem concept that Tansley had proposed in 1935. This concept, for Odum, was to be a powerful model that would be applicable to any study; he had only to define it clearly. However, Odum experienced numerous problems. Many of the examples he gave did not seem to have clear boundaries (for example, a meadow has fauna that roam in and out). Other examples, like the lab micro-ecosystem and a spaceship environment, were not even natural. This made an ecosystem anything that acted as a self-contained life support system. As Worster noted "[a]ll ecosystems ultimately became, by that logic and definition, nothing more than abstractions in the minds of ecologists."[13] This conclusion certainly was not conducive to the development of a universally applicable model. Although the idea that an ecosystem is nothing more than an abstraction in the mind of the ecologist seemed to worry Worster, there was another, more serious, problem with this definition of an ecosystem: nothing, not even a spaceship or a laboratory, is really fully self-contained. At some level, then, the most important and basic unit in ecology rebelled against its own definition.

---

[12] Cooper (1965), 388.

The problem of defining the units of investigation was given another twist in the mid-1990s. Plant ecologists had been wrestling with the problem of associations, association types, successional stages and climax stages since the early 1900s. Arguments and disagreements most often consisted of attempts to refute other positions by suggesting alternative interpretations of the data. In 1995 another position was added into this mix. Frelich and Reich suggested that a forest would be a different forest depending on the scale from which the researchers chose to study it. For example, the fir-spruce-birch forest "may be a uniformly mixed forest with individual trees of different species next to each other (the result of convergent succession), or a series of small mono-dominant stands (the result of divergent succession)."[14] In effect, they argued that what the forest was depended on the level from which it was observed. This recognition is difficult to reconcile with science's requirement of neutral observation of an independent reality. If the identity of the forest depended on the level of observation, then the ecologist played an important role in determining the forest.

---

[13] Worster (1996), 366.

[14] Frelich and Reich (1995), 327.

A particularly interesting example of the difficulty ecology encountered in defining its object of study comes from Hayward's 1948 study of the Wasatch chaparral in Utah. One of the purposes of Hayward's study was to locate the boundary of the chaparral. After reviewing the literature on chaparral studies, Hayward concluded that hitherto no one had been able to accurately locate it. "The position of the chaparral," he noted,

> among the biomes of North America as proposed by a number of bio-ecologists is somewhat peculiar. From the point of view of its dominant plants and general appearance it forms a distinctive and striking formation, but when all of the subdominant plants and the great majority of animals are taken into consideration, it possesses many of the qualities of an ecotone.[15]

The peculiarity Hayward noted indicates the slippage of the chaparral; it does not appear in the same location or as the same entity for each researcher. Hayward observed that "certain peculiarities of the Wasatch chaparral make it extremely difficult to classify under the terminology employed by Clements."[16] Hayward referred to this difficult community as the one "lying between the northern desert or pinion-juniper and the montane forest."[17] Here the chaparral was defined by what lay around it and the species that comprised it, except that these did not remain consistent. Hayward then cited the work of other ecologists, which described this community as the sub-montane shrub zone immediately above the pinion-juniper in which neither the yellow pine nor oak are considered dominant (Graham, 1937), an oak sub-climax of the yellow pine climax (Dixon, 1935), a community in transition as oak replaces yellow-pine in some places

---

[15] Hayward (1948), 498. An ecotone is a transitional area between two other well-defined associations or formations. Hayward was saying that the chaparral is either well defined or it is not, depending on whom one asked.

[16] Hayward (1948), 494.

[17] Hayward (1948), 494.

(Tidestrom, 1925), and as the oak-mountain mahogany zone (Daubenmire, 1943) whose "true position lies in a 'broad belt between the juniper-pinion and desert regions.'"[18] However, in contrast to Daubenmire, Hayward felt that "the chaparral never occupies such a position but lies above the juniper-pinion wherever the two communities are present."[19]

The list of conflicting positions on the location of this community (which actually had no consistent name) continued and included Rasmussen (1941), Ramaley (1931), and Clements (1920). After citing these seven authors who disagree on the location of the chaparral, Hayward ended by saying that "the *normal position* of the chaparral is in contact with the grassland below and with the juniper-pinion or montane forest above."[20] One wonders how it is possible that something with so many different locations and different interpretations can be said to have a normal position. The closest Hayward came to acknowledging this question occurred two paragraphs later when he argued that "sufficient examples have been given to indicate the complex problems associated with the proper placement of the community in the scheme of bio-ecological classification."[21] Hayward concluded his article with the passage quoted above that discussed the region's status as a formation or an ecotone, but added the rather unconvincing comment that "[i]n light of these latter considerations it is here considered as an ecotone."[22] Hayward's conclusion appears far from inevitable. The chaparral did not proclaim itself to Hayward without a great deal of work on his part. Yet, his role in shaping the chaparral was minimized because his explanation rested on a version of nature 'out there.'

---

[18] Daubenmire, quoted in Hayward (1948), 495.
[19] Hayward (1948), 495. This stance, Hayward added, is also supported by Tanner and Hayward (1934) and Behle (1943). For a similar type of discussion, but not regarding chaparral, see (Wells 1962) who cited three different and conflicting explanations for the forests and grasslands in the San Luis Obispo quadrangle in California.
[20] Hayward (1948), 495, emphasis added.
[21] Hayward (1948), 495.
[22] Hayward (1948), 498.

Thus far, these examples pertain to single studies, that is, authors noting the confusion and contradictory nature of their topic. These problems of determining the unit of measurement and even the location of the object under study plagued ecologists for most of the 1900s. The problem, however, was not confined to individual works. The debate over climax communities will serve as an example of the problem of determining units in nature, although in this case it is played out through a number of articles. In some articles the climax community and the succession leading to it were seen as obvious and visible. For example, Beckwith claimed that "[p]lant succession usually proceeds from one stage to the next in an orderly manner."[23] Compare this with Whitaker's comment only one year earlier: "[s]uccession may thus be thought to occur, not as series of distinct steps, but as a highly variable and irregular change of populations through time, lacking orderliness or uniformity in detail, though marked by certain fairly uniform over-all tendencies."[24] In another case, Shantz suggested that "true climax is probably hypothetical since the habitat is in a continuous process of cyclic change and possibly of a direct drift in one or several directions, and since use by animals is not constant and continuous but varies in intensity and kind."[25] In the end, succession to climax was seen as (1) orderly and predictable, (2) highly variable and irregular (but still occurring), or (3) hypothetical. It seemed that ecologists could not decide whether the concept was simple, complicated, or hypothetical. It was as if nature refused to remain constant for the different investigators.

---

[23] Beckwith (1954), 354.
[24] Whittaker (1953), 44.
[25] Shantz (1940), 316.

It might be argued that *association*, *association type*, *climax*, *succession*, and so on are particular and specific scientific terms and, as such, are not 'out there' in nature. Of course these concepts are impressed upon nature, one might contend; nature is being interpreted through the science of ecology. If this contention is true, then the problem is not that concepts are imposed; the problem is that nature is not understood well enough to permit the development of predictive models that faithfully represent what is really out there in nature. The solution, then, would be to refine our understanding of nature and develop better and better models that would more and more faithfully capture nature's true colours. As long as the scientific method was followed, the argument went, the mysteries of nature would be penetrated.

This is the temptation of the developmental, progressive, and modern science that I outlined in Chapter Two. For the most part ecologists adopted this view of how knowledge developed, was added to, and built upon. This conceptualization of the way ecology developed used the scientific method to combat the relativistic undertones in the idea that nature is not 'out there' awaiting human discovery and study. However, when the only two options (that is, nature is 'out there' or 'in here') are framed as exclusive, then any convergence of them is not easy to acknowledge or accept. In other words, the idea that nature may not be simply 'out there' is taken as an argument for it being completely constructed by culture. Likewise, when nature is seen as being 'out there' this can be criticized as naively positivistic. This polemical stance precludes productive exploration of the exchanges that could occur between poststructural accounts of the power of language to produce reality and the power of science to understand and work with reality. This preclusion becomes evident when one looks at what happened as ecologists began to encounter instances where nature was not easily classified as 'out there.' Many of the exchanges between a nature 'out there' and one that was 'in here' were incorporated by ecologists into the structure of scientific discourse. Those exchanges that could not be assimilated were rejected and appear as a silence or blind spot. This blind spot can be seen in Hayward's dismissal of the differences in interpretations of the location of the chaparral as unimportant or incorrect. The idea of a progressive science limits researchers' ability to accept competing explanations and claims. A similar kind of empty space appeared in Cain's discussion of the various meanings of natural areas when he concluded that they were definable as indefinable. The turn to the scientific method for legitimation and justification was beneficial to ecology in many respects, but it also had its drawbacks. Specifically, the scientific method, as employed by ecologists, made it easier to overlook the evidence it was generating that undermined the belief of a place 'out there' in which nature hides.

Poststructural theory and science have been seen as being at odds with each other in some respects. The criticisms poststructural theory was subjected to suggest a certain level of antagonism between the scientific perspective and a poststructural one. However, I contend that this antagonism is not necessary. Poststructural theory can contribute insights into the natural sciences without at the same time undermining the power, value, and place of that category of land we call wilderness. Instead of focussing on the negative or conflicting points of contact, I focus on the productive points of contact. Three places of such contact offer hints into the ways ecology and OR can begin to incorporate poststructural insights to strengthen their position. These three points concern traces and différance, ghosts or spectres, and undecidability.

The splitting of humans from nature was not so much wrong as ill-conceived. This ill-conception began the moment ecologists and OR scholars thought natural nature was pure; that is, it was a place or a process(es) without the presence of the human presence. They thought, we might say, that this represented true nature, its essence, its core characteristic. Certainly, both ecology and OR recognized that nature often bore the marks of humanity: a garden, for instance, would be natural but not pristine. It could not be wilderness. It did not illustrate the workings of many natural processes (for example, the predatory-prey relationships in large mammals). In order to understand or experience these kinds of relationships, both ecology and OR suggested we need a particular type of nature, a natural nature devoid of human presence.

As part of the larger project that produced this need for natural nature, the discourses of ecology and OR also disciplined and ordered the artefacts within their respective fields. Chapters Two and Three were devoted to an analysis of this ordering that showed it to be part and parcel of modernity itself; thus, artefacts in ecology and OR were produced in accordance with modern scientific practices. These artefacts were arranged in systems of legitimation and located at specific (although shifting) positions within the power/knowledge nexus of the modern disciplinary regime. Notions of authenticity, pristineness, and balance were particular focal points in this power/knowledge nexus. These points formed the structural anchor that did not change even while the discourses of ecology and OR changed. Some of these changes were charted to illustrate the unchanging characteristic of the structure that produced that knowledge; for example, the structural anchor of authenticity did not change even as there were many changes in how this authenticity manifested itself. This structural constancy is the centre of natural nature that allows for the play of meanings within the discourses of ecology and OR but that refuses to change itself.

The problems engendered by attempts to get and keep people out of natural nature can be understood through certain poststructural concepts: the trace and différance, ghosts or spectres, and undecidability. These problems can be understood as producing ghosts or spectres that leave behind paradoxical traces that continually frustrated the very project to produce natural nature in the first place. Whenever researchers claim to have encountered a pure nature that lies outside human culture, they discover that they have already been there. How can one know where a limit lies unless one has probed it, crossed it, or transgressed it? Thus, to know nature as outside humanity, humans must have already crossed that line. Paradoxically, knowledge of natural nature was produced in an encounter between it and humanity; thus, humans are always already present in natural nature. Past the limit of humanity (for example, past the line on the ground separating the inside of the quadrat from the human-tainted world outside it) ecologists found themselves already present in oddly paradoxical ways. The structure of pure (human-less) nature was only accessible through the deliberate action of ecologists. Through this forced absence, ecologists encountered a spectral presence. Humans had already been there surveying the land, laying out quadrat lines, walking on it, and marking off what was natural nature from human-influenced nature. This occurred in OR, as well: people found themselves always already in the forests they studied in the shape of fire suppression policies, irrigation plans, trampling effects, and so forth.

Conceptually, the always already presence of humans happened as well. The models developed in ecology and OR were, by definition, anthropomorphic and yet these models claimed to represent nature in its true form, that is, without humans. It is a paradoxical notion that an anthropomorphic model could accurately represent non-anthropomorphic systems, not least because the more it succeeds, the more it finds itself being mirrored in nature. Thus, the more accurately it works, the more it finds nature to be like itself, that is, anthropomorphic and, thus, hardly a natural nature at all. The result is that nature becames *less* natural when seen through anthropomorphic models.

Not all manifestations were as indicative of human presence as a quadrat line. Lurking on the sidelines was a shadowy figure, barely visible, of the thing ecologists and OR researchers were attempting to remove. In many ways, the clearer remnants, such as footprints, worked to further mask this spectral figure. It was fairly easy for ecologists to eliminate footprints from the quadrat and in doing so it appeared as if the presence of humans was effectively removed. As we saw, however, it did not prove so easy to remove all traces of humanity; so, more elaborate methods were devised that pushed this shadowy figure farther and farther from the spotlight and into the murky darkness of shifty/ing language, scare quotes, and paradoxes. These strategies proved an effective method for dealing with the fact that the splitting off of people from natural nature was not (could not be) successful enough to justify continual and unproblematic use of this separation. The confusing use of scare quotes, for example, made it increasingly difficult to see the shadowy figure of the human presence amidst this dark and murky background until it disappeared altogether.[26] Or so it might be thought. In actuality, the shadowy presence of humanity remained. If anything, because it went unrecognized, it had more freedom as its effects were neither accounted for nor studied.

---

[26] It is quite possible that in addition to making this figure more obscure, these techniques made the figure's background more murky, which also contributed to the difficulty in seeing the spectre, but so too did it cloud the object of its own study. In short, the more scare quotes and other techniques were used, the harder it became to understand just what they were referring to.

This shadowy figure that lurks underneath or as a palimpsestuous component of the discourses of ecology and OR can be fruitfully understood through the trace, différance, spectres, and undecidability. These are the excesses that overflow our accounts of natural nature, spill into the spaces between our words, and hide in plain sight within our very texts and on the land itself. In this chapter I try to achieve a more explicit encounter with this figure. While explicit encounters with this figure are uncomfortable (largely because no [easy] solution presents itself afterward), they do gesture toward a structural openness from which a potentially more ethical response can issue.

What is the Trace?

Nothing is directly observable. ~ J. W. Grove[27]

The trace is absent presence, or perhaps a present absence: "the trace is Derrida's name for what is never there."[28] The trace cannot and, therefore, does not simply exist. And yet, because whatever has been traced necessarily must have been evicted, it will always have left its mark. As a suddenly abandoned house leaves signs of its occupants, what has been traced betrays signs of its absence. The trace exists as a negated presence. This trace is not an absent signifier: words, concepts, and ideas can be present or absent. We can forget something we once knew. We can omit a portion of what was said. The trace, however, is (not) something else.[29] "The trace is not a presence but is rather the simulacrum of a presence that dislocates, displaces, and refers beyond itself. The trace has, properly speaking, no place, for effacement belongs to the very structure of the trace; otherwise it would not be a trace but an indestructible and monumental substance."[30]

---

[27] Grove (1989), 8.
[28] Neel (1988), 150.

The trace can be likened to the grooves left in the Mystic (or Magic) Writing Pad toy. The pad is transparent celluloid over a sheet of grease-proof paper, which itself is over a dark wax layer. When a stylus is pressed into the celluloid, the force makes grooves in the underlying wax layer. The darkness of the wax layer shows through in the paper layer in the middle. When the paper and celluloid are lifted, the writing disappears. The lines and marks are not actually on the paper or the celluloid, nor are they on the waxed layer: only grooves are on the wax. The pressure exerted on the pad causes the writing to appear. Pressure is the cause; the effect is the signifying lines and symbols on the paper. These grooves, which are not writing, are nevertheless, the sign of writing. There is a type of absence here, not of meaning, but of presence. The lines and marks on the wax are not meaningless, but writing is not exactly present either. We have access to the lines only, not to the force that created them. This force is the movement of signification. [31]

If all signifying practices (for example, words, gestures, images, concepts, and so on) point away from themselves, but never reach their referents, then meaning resides in the process of pointing at or pointing toward, not in arriving. Signification operates on a centrifugal logic as distinct from a centripetal one. Meaning is always toppling onward and outward (what Derrida called *dissemination*[32]) instead of returning on itself and being anchored by a solid referent. In short, that which words refer to is not self-present in those words. Meaning always implies further meanings in an endless movement outward from sign to sign. The referent never manifests itself. According to Derrida, "what is postponed is never recovered and what is invested in never redeemed…. We must conceive of 'an expenditure without reserve' and 'an irreversible wearing-down of energy.'"[33] Because signs are supposed to stand in for the thing itself, they defer presence indefinitely. "When we cannot take hold of or show the thing, let us say the present, the being-present, when the present does not present itself, then we signify, we go through the detour of signs. We take up or give signs; we make signs. The sign would thus be a deferred presence."[34] As such, signs must also differ from the presence of that to which they refer. There is, then, both a differing and a deferring of presence by signification. Signs are spatially and temporally distinct from self-presence, the being present in itself.

---

[29] Derrida sometimes uses parentheses, strikethroughs, and other textual signs to mark certain words as problematic: they should be read as both a part of the text and apart from it. They are neither there nor not there; they are both there and not there; they are either there or not there.

[30] Derrida (1973), 156.

[31] This example of the toy writing pad comes from Harland (1988). The process of signification and its implications was discussed in more detail in Chapter One.

So signs are not presence. Then does this presence not exist? This question actually obscures the problem. The question of presence cannot be answered in any way other than with reference to the trace. "As any signifying system operates, the trace of the now-gone pure knowledge and communication play in and around the system all the time. But the system depends both on those purities being absent and on the play of traces of those purities whose promised arrival keeps signification in operation—infinitely."[35]

---

[32] See Chapter One for a more detailed discussion of dissemination.
[33] Harland (1988), 148.
[34] Derrida (1973), 138.
[35] Neel (1988), 151.

Without the promised arrival of pure knowledge, signification would collapse onto itself. Signification works on promise.

There can be no signified that does not take this detour through signification. To do so is to posit a transcendental signified that can meaningfully exist outside all signifying practices.[36] "What (dis)appears in place of the transcendental signified is the trace, which creates the transcendental both by never appearing, so as not to become the transcendental signified itself, and by replacing the transcendental signified by its own constant movement."[37] This non-appearing can be taken as the transcendental signified's functioning. The necessary absence, the present absence, creates the non-space of the transcendental signified that manifests as the trace.

So traces are deferred presence and they differ from presence. What brings about this distinction? How does presence differ from the trace of presence? What causes this difference? What produces the very possibility of difference itself (as opposed to the difference between already determined things)? Differences are an effect of that which makes difference possible. However, they are

> effects that do not have as their cause a subject or substance, a thing in general or a being that is somewhere present and itself escapes the play of difference. If such a presence where implied (quite classically) in the general concept of cause, we would therefore have to talk of an effect without a cause, something that would very quickly lead to no longer talking about effects. I have tried to indicate a way out of the closure imposed by this system, namely, by means of the 'trace.' No more an effect than a cause, the 'trace' cannot of itself, taken outside its content, suffice to bring about the required transgression.[38]

If causes must of necessity be things (subjects, substances, events, and so on), how can some*thing* cause the possibility of difference itself? As the cause of difference, it must precede all differences. But how would this 'thing' itself differ from all other things, as it must to come before the differentiation of all things? The problem, as Derrida noted, is that we can say differences exist, but if all existence is to have a thing as its cause, then whatever causes difference itself must both exist and yet not be anything; it must be an effect without cause. The trace is the escape from this closure.

---

[36] Ecology and outdoor recreation posit pristine and pure nature as a transcendental signified.
[37] Neel (1988), 150.
[38] Derrida (1973), 141.

Closely linked to the trace is Derrida's notion of différance. Différance can be understood as the non-full non-simple origin of the trace. Like the trace, différance cannot be exposed, for we can only expose that which can manifest, become present, be shown—a being-present in its truth.

> However, if différance is (I also cross out the 'is') what makes the presentation of being-present possible, it never presents itself as such.... Holding back and not exposing itself, it goes beyond the order of truth on this specific point and in this determined way, yet is not itself concealed, as if it were something, a mysterious being, in the occult zone of a nonknowing. Any exposition would expose it to disappearing as a disappearance. It would risk appearing, thus disappearing.[39]

Différance cannot be exposed, but it cannot be hidden either. Only things can be hidden. If différance is not a thing, it cannot be hidden. It cannot come into existence not because it is too well hidden, but because it exceeds the bounds of the categories *exposure* and *presence*. What presents itself instead is the trace of presence. The trace is the mode of manifestation of presence that différance puts into play.

The trace and différance are more obviously active in metaphysical works that discuss the ultimate nature of reality. We must make a distinction here between Being and being. According to Heidegger, Being must always present itself as *a* being, as this or that instance or manifestation of Being. It is Being that which allows beings to present themselves in their particulars. This is the difference between presence and present. What are present are beings; what is not is the presence of Being. Being is always only traced in beings. Unfortunately, we seem continually to forget this distinction between Being and being. We make, in other words, what Derrida has called the mistake of logocentrism. Furthermore, this forgetting itself goes without notice. The forgetting gets forgotten. It "has disappeared without leaving a trace. The very trace of difference has sunk from sight. If we admit that différance (is) (itself) something other than presence and absence, if it *traces*, then we are dealing with the forgetting of the difference (between Being and beings), and we now have to talk about a disappearance of the trace's trace."[40] In this way, metaphysical texts pretend to expose Being; they must deny (and/or forget) the necessary detour into signs and signification. Instead of such a circuitous route, they claim (without ever doing so) that Being is self-present in their texts.[41]

---

[39] Derrida (1973), 134.

Bracken explains Being in the following manner.

> A thing is anything that in any way is. Yet when one says that 'a thing is,' the 'is' that allowed it to *be* in the world goes unthought.... Thus to inquire into the presence of what is present, or the Being of beings, means asking about the 'is' that is presupposed in any discussion of what exists.... The 'is' is a nonthing that permits things to be: Being holds beings in place without being anything itself.[42]

Yet the gift of Being of itself as the region where the unconcealment of beings occurs actually hides Being: it is a gift that never arrives, for Being *never* appears as anything. The nonplace whence Being offers its gift is also the nonplace where one 'finds' différance. And the gift that Being offers through the play of différance is the nonthing of traces of itself. This trace is, finally, the residue of Being that manifests itself as an absence in the various beings in signification.

---

[40] Derrida (1973), 155.

[41] This claim can operate even in texts that present the presence of différance: if différance is anything, it is that it isn't. What makes Derrida difficult to understand is that he recovers the second forgetting. He remembers and reminds us that we must always have already forgotten the distinction between Being and being in the act of writing. This forgetting is the condition of possibility for all writing (and signification in general).

[42] Bracken (1997), 28.

Forgetful texts are so because they often search for the ultimate meaning of reality, which can only be found through a forgetting. Derrida has remarked

> how every last concept and category of Western philosophical thought … can be traced back to some effect of sublimated metaphor, some figural expression whose root meaning philosophy must needs forget or repress if it is to keep up its own constitutive self-image as a discipline specialized for adjudicating issues of argumentative warrant, truth and falsehood, knowledge and disbelief, the intelligible *versus* the sensible, and—subsuming all these—its claim to determine the very 'conditions of possibility' for separating erroneous from 'clear and distinct' (or philosophically valid) ideas.[43]

A search for the origin of meaning leads to metaphor, which cannot be the source of meaning because metaphor operates on a distance. The trace, then, "is in fact the absolute origin of sense in general. Which amounts to saying once again that there is no absolute origin of sense in general. The trace is the différance which opens appearance and signification."[44] Through différance and the trace a (non)space opens for the unconcealment of Being as being. What appears cannot do so without an opening in which to manifest. However, what gives this place of unconcealment? The trace of Being marks the play of différance (of presence), which in turn is the nonplace of the manifestation of beings in discourse.

The discourses of ecology and outdoor recreation resemble these metaphysical works to the extent that they discuss natural nature as a transcendental signifier. Wilderness, pristine nature, and undisturbed ecosystems are taken to be a specific form of reality as it really is; these are the ultimate shape and functioning of the natural world around us, transcendental signifiers, the Being of nature as it were. Deconstruction shows the gaps, contradictions, and excesses that must be suppressed or denied in order to see structure in this absolute sense. These gaps and contradictions occur, Derrida argued, "wherever reason looks for a ground or authenticating method immune to the snares of textuality."[45] This argument applies not only to Western metaphysical and literary texts. Ecology and OR, likewise, refuse to acknowledge their textuality. Eugene Odum's universal energy model provides an example of this.[46] For Odum, this model was simply the structure of natural nature; it was never interrogated as part of a textual practice that must deny the slippage of meaning it cannot account for or contain.

---

[43] Norris (1998), 141.
[44] Derrida, quoted in Neel (1988), 152.

When ecologists argued about the role that disruptions play in the sequence of succession, they were arguing over the original purpose and functioning of an area. What was the natural sequence of change this area would undergo were it to realise itself fully without the interruptions of humans? Likewise, in OR, discussions about the original state of a forest before humans altered it by camping were common in the discourse on minimum impact camping techniques. In other words, ecology and OR are premised on the metaphysical assumption that one can come to know the ultimate nature of natural nature, and, indeed, that an ultimate nature of natural nature exists to be known. There is actually a double logocentrism here. There is a belief that natural nature can be made manifest, but there is also a belief in the wider idea that science can come to know the world (be that a natural one or not) directly (that is, unmediatedly) without taking the detour through signification. This second type of logocentrism is at work when scientists argue they know the real effects humans have on nature. In large measure, it is this practice that lends such power to scientific discourse. When scientists claim to have found *real* effects, they align themselves with very powerful discursive forces that construct a type of knowledge that simultaneously denies its own construction. When presence (of nature) is seen as self-present, doubting its presence becomes preposterous.

---

[45] Norris (1998), 30.
[46] Discussed in Chapter Two.

Looked at closely, however, ecology and OR present the being of nature, not Being. They forget the very distinction between Being and being; instead, they assume that they have access to a transcendental signifier. As with texts on metaphysics, ecology and OR forget this tracing of presence. The trace of Being is itself removed such that scientists conclude they have reached the ultimate nature of nature. The trace is no longer remembered in these works. They claim that there is no difference between what they state and the Being that has already given beings the opportunity to manifest themselves for these texts. Nonetheless, being forgotten is not the same as not being present. The trace cannot be removed (mostly because you cannot remove what cannot present itself); it can only be forgotten. Thus, for Derrida, the forgetting of the trace and its presentation as a nonthing are similar:[47]

> [t]he effacing of this early trace … of difference is therefore 'the same' as its tracing within the text of metaphysics. This metaphysical text must have retained a mark of what it lost or put in reserve, set aside. In the language of metaphysics the paradox of such a structure is the inversion of the metaphysical concept which produces the following effect: the present becomes the sign of signs, the trace of traces. It is no longer what every reference refers to in the last instances; it becomes a function in a generalized referential structure. It is a trace, and a trace of the effacement of a trace.[48]

It is in this confusing realm of the effaced forgotten presence of Being that ecology and OR find themselves when they attempt to explicate the nature of natural nature.

---

[47] Forgetting the trace and presenting it as a non-thing are similar except in the all-important sense that only the latter of these processes can be made conscious.

[48] Derrida (1973), 156.

And yet, we *can* trace this effacement and forgetting. We must, of necessity, have already traced presence, but, retroactively, we can remember this. We cannot un-trace it, but we can realise that we have traced it already. In this manner, the absence becomes present; the trace is no longer effaced or forgotten. And yet, the more absence becomes present, the greater the chance we have of committing the very same forgetting that Derrida accuses metaphysical texts of perpetrating. Have we not just forgotten this forgetting and effacement? Cannot metaphysical texts be read and understood as "the trace simultaneously traced and effaced, simultaneously alive and dead?"[49] If so, we acknowledge the trace as contradiction or paradox without resolving or even dwelling on it. The challenge for ecology and outdoor recreation is a similar one of forgetting the/and tracing. Can we not read ecology and OR texts in a manner similar to the way that Derrida advises we read metaphysical ones? That is, can we invoke the paradox of a specific presence of Being (of natural nature) without resolving it through a forgetting that is a resolution that denies the original paradox of Being and traces?

When nature is conceived of as a pure presence, when it is that which is authentic in an original sense, then the operation of the trace can be made more visible when we refuse to forget. Nature is traced within itself because pure nature in and of itself (that is, without any human influence) can never fully appear. This hiding is absolute and at the same time impossible. Natural nature, more than many aspects of discourse, is expressly self-present, although we now know this cannot be made manifest. The desire to bring nature into the full disclosure of self-presence cannot be real-ised. This is the always already traced presence in the signification of nature. It is the present absence (deferral) of presence in signification—this (non)movement of différance—that 'causes' the trace of natural nature.

---

[49] Derrida (1973), 156.

When we think about natural nature in a logocentric way, we mistakenly suppose that it manifests as a transcendental signifier. However, we know that the transcendental signifier cannot *be*, because it must take the detour through signification and therefore cannot arise above signification to become transcendental. However, this requirement does not mean that the transcendental signifier is fully absent. If there cannot be any transcendental signifieds in the first place, then they cannot be removed. To remove something that was never there makes little sense. So to speak of whether or not self-presence exists, whether a transcendental signified nature or an unmediated reality exists is to ask a faulty question. To ask 'which came first, physical existence or discourse and meaning' is to ask a question based on the metaphysics of presence. It is a logocentric question because it requires something either just to exist or not. When one examines the way in which natural nature appears in the literature of ecology and OR, one can see the attempt at manifesting such a transcendental signified. Wilderness and natural ecosystems are positioned in these discourses as pure places or processes without human influence; however, we are now in a position to better understand that transcendental signifiers actually never arrive. Thus, even when the discourse expressly claims that wilderness is a true and authentic place, we can see this as a logocentric claim that denies the functioning of the trace.

We should be wary of a temptation here. When we work within a binary framework that allows for only two options, it is likely that we will understand the absence of the presence of nature as an endorsement of nihilism or relativism. When only two options exist, if we rule out one, we must end up with the other. When the options presented are either ontologically absolute knowledge or relativism, we unnecessarily limit our choices. Even though we cannot go back to the texts in ecology and OR in order to remove the denial and overcome the suppression, we can nevertheless conceptualize nature by remembering this necessary removal and try to understand the implications of it. In other words, our inability to render natural nature fully present does not mean that we must forever lose it completely. This type of either/or thinking cannot be applied to différance and the trace.

Ghosts in/of Nature

There could be a ghost above my head right now as I talk to you
~ Melissa Auf der Maur

The 'real world,' however one has hitherto conceived it—
it has always been the apparent world once again
~ Friedrich Nietzsche[50]

Whereas the trace and différance apply to all cases of signification and cannot manifest as anything in particular, the concept of haunting or spectrality is more restricted. The trace may be thought of as

> *the différance* which opens appearance … and signification. Articulating the living upon the nonliving in general, origin of all repetition, origin of ideality, the trace is not more ideal than real, not more intelligible than sensible, not more a transparent signification than an opaque energy and *no concept of metaphysics can describe it.*[51]

The trace, then, opens all appearances and all signification. Without the trace there can be no being; by contrast, spectres or ghosts refer to a particular form of something or someone no longer present, but not quite absent, either.

---

[50] Nietzsche (1967 [1901]), 305.
[51] Derrida, quoted in Joseph (2001), 97.

Spectres or ghosts are, as Derrida says, becoming-bodies, bodies not fully formed but in the unending process of forming. "But, unlike *différance*, these concepts refer to a specific level of meaning within the social, a level that is, precisely, phantasmatic, spectral, mystical."[52] Derrida speaks of the spectre as a returning or *revenant* of what once was. It is neither body nor soul and both body and soul.

> For it is flesh and phenomenality that give to the spirit its spectral apparition, but which disappear right away in the apparition, in the very coming of the *revenant* or the return of the spectre…. one does not know what it *is*, what it is precisely. *It is* something that one does not know, precisely, and one does not know if precisely it *is*, if it exists, if it responds to a name and corresponds to an essence. One does not know: not out of ignorance, but because this non-object, this non-present present, this being-there of an absent or departed one no longer belongs to knowledge. At least no longer to that which one thinks one knows by the name of knowledge. One does not know if it is living or if it is dead.[53]

The ghost is a 'thing' that cannot be and yet is visible. It is nothing made visible. It is no thing that we can see. In this sense, ghosts are akin to traces: both challenge the metaphysics of presence. That is, both question whether being is properly thought of as only presence along with everything that organizes itself through this (for instance, a visible thing, as a temporal moment—the present or the now, the presence of a substance or essence, the self-presence of the *cogito* and the consciousness, the co-presence of self and other, and so forth). Consequently, Derrida uses 'hauntology,' a play on 'ontology,' to refer to "a trace of voices, epistemologies, and temporalities that haunt history and awareness, where the past, present, and future come together."[54] Although both traces and spectres are similar in this regard, what interests us about spectres and haunting exceeds this affinity.

---

[52] Leledakis (2000), 181.
[53] Derrida (1994), 6.
[54] Tavin (2005), 101.

In addition to the puzzle of whether ghosts are real or not, they pose an interesting temporal dilemma. Derrida calls the spectre the *revenant* or the coming again; it "comes by coming back [*revenant*], it figures both a dead man who comes back and a ghost whose expected return repeats itself, again and again."[55] We cannot control the coming and going of spirits. The dead man comes back as a ghost without our permission and the first time we encounter it, it is already a *revenant.* Ghosts are the past returning in the present. They are the past's future and in this sense also a type of future for the present—we all could become ghosts. Ghosts trouble the division of real and fictional by simultaneously troubling the temporal division between past and present.

> This prompts us to think of the here and now as occurring within a time and space that is dis-adjusted with itself. As an apparition, the ghost is neither here nor there, neither then nor now, marking its absent presence in more and less than one place and time. Derrida meticulously draws out the implications of such a disjunction by offering a reflective reading of Shakespeare's *Hamlet,* in particular Hamlet's dilemma 'to be or not to be' in the face of an inherited responsibility. Hamlet's confrontation with the ghost of his father precipitates the judgment that 'The tim[illegible] out of joint' and it becomes his responsibility, as his father's heir, to set things right.[56]

This is one of the reasons why spectres are troubling. They threaten our sense of time and our sense of place, neither of which we are in the habit of questioning or considering as unstable/tentative. Derrida asks us "[w]hat is the time and … history of a spectre? Is there a present to a spectre? Are its comings and goings ordered according to the linear succession of a before and an after, between a present-past, a present-present, and a present-future, between a 'real-time' and a 'deferred time'?"[57] How should we respond to these questions? There seems no clear answer. Indeed, this is the lesson of the spectre: answers depend on a metaphysics that spectres disrupt. There is an undecidability about them. We cannot know for certain: We cannot decide one way or the other. The question goes either way or both ways and it goes either way *and* both ways. Is it alive? Yes, or no, but also yes and no (and, one might add, spectres exceed this binary altogether).[58] Derrida insisted on the logic of the ghost because it pointed "toward a thinking of the event that necessarily exceeds a binary or dialectical logic, the logic that distinguishes or opposes *effectivity or actuality* (either present, empirical, living—or not) and *ideality* (regulating or absolute non-presence). This logic of effectivity or actuality seems to be of a limited pertinence."[59]

---

[55] Derrida (1994), 10

[56] D'Cruz (2006), 66. See also Fraser (2000), 777-778 and Luckhurst (2002), 533.

[57] Derrida (1994), 39.

The undecidability of ghosts places ethical responsibilities on those who converse with them. The answer to spectres is not given; it is made. And making decisions such as these are violent affairs. We are subjected to this violence just as we subject spectres to it. Hamlet inherited the responsibility to set things right and this involved violence. A ghost weighs on those of us in the present who "have to answer for it. *To answer for the dead, to respond to the dead.* To correspond and have it out with … obsessive haunting, in the absence of any certainty or symmetry. Nothing is more serious and nothing is more true, nothing is more exact."[60] As with Hamlet, in ecology and OR ghosts bequeath to us a weighty responsibility to interact ethically with the environment. They also bequeath to us the responsibility to respond to our history. How we have interacted with nature in the past gives rise to ghosts today that demand we account for our past now, in the present. This is a charge that we must answer to, have out with, correspond with, or respond to.

---

[58] See, also, Thurschwell (2004) who notes that "the specter represents a certain undecidability between the living and the dead, the present and the absent, and the imagined and the actual" (19).

[59] Derrida (1994), 63.

[60] Derrida (1994), 109. Derrida notes, too, that "to weigh is also to charge, tax, impose, indebt, accuse, assign, enjoin," 109.

This responsibility is not one of unravelling the secret of the ghost. Ghosts remind us of secrecy itself, of the inability to know, with certainty, what they are. Ghosts "may open us up to the experience of secrecy as such: an essential unknowing which underlies and may undermine what we think we know."[61] A ghost

> occupies the place of the … Other: a wholly irrecuperable intrusion in our world, which is not comprehensible within our available intellectual frameworks, but whose otherness we are responsible for preserving. Hauntology is thus related to, and represents a new aspect of, the ethical turn of deconstruction which has been palpable for at least two decades.[62]

We must respect the spectre from the past in our present and accord it a place justified by the wavering presence of such an apparition. We cannot re/solve this apparition into existing frameworks without at the same time dissolving it. Respecting the Otherness of ghosts helps remind us of the Otherness of nature. This is not the same as letting nature speak for itself, of listening for what nature is on its own terms, for we can only hear in languages of presence, which cannot abide ghosts.

Using the figure of ghosts, we can now see them haunting ecological language. Nature was seen as progressive, developmental, and stable for most of the 1900s. However, with the advent of newer modelling in ecology we are revising our estimations of the stability and developmental aspects of nature. There is a type of Otherness that is denied when nature is conceptualized as completely comprehensible. A remainder always existed, even as ecologists sought comprehensive models and theories that explained nature. Some parts, certain elements, and particular processes were always left out of their explanations. This is the Otherness of natural nature. It is that part that is profoundly and irrevocably other to humans. The element in nature that refuses to give itself up to rational inquiry is precisely what must be preserved. Nature is ghostly in its secrecy. The value of ecological science lies not in unravelling this secret for all to see. Its value lies in clearly pointing out the unsolvable secrecy of nature. The same is the case for OR literature. We cannot ever hope to know wilderness without at the same time misunderstanding those portions of it that always lie outside our analytical frameworks, and, we must admit, that lie outside the capacity of all analytical frameworks.

---

[61] Davis (2005), 377. Davis explains this secrecy as "the structural openness or address directed towards the living by the voices of the past or the not yet formulated possibilities of the future. The secret is not unspeakable because it is taboo, but because it cannot not (yet) be articulated in the languages available to us," 379.
[62] Davis (2005), 373.

Curiously, the most common ghost in ecology and outdoor recreation is a human one. It is the spectral presence of humanity itself that manifests its ghostly appearance. There is a hole in ecology and OR where the human figure should be. It is a human-shaped hole filled by a human spectre. For instance, when the Ecological Society of America met in Chicago in 1933, a discussion took place in which Dr. Higgins expounded on an international commission that studied the relationship between herring, tides, and plankton in the Bay of Fundy. Dr. Higgins mentioned that the commission was concerned about the consequences of a hydroelectric dam. The commission concluded that the dam would virtually destroy the fisheries in the bay but would have little effect outside this area. This might sound significant and worthy of further comment; however, this dam question received only one sentence. Immediately after noting the effect the dam would have, Dr. Higgins spent a paragraph discussing what he called the important problem: plankton brought into the area and its relation to the herring population. Everything, he said, "brings us back to the old question of the amount of plankton and the reason." Unfortunately, in the 1930s, our knowledge of plankton was insufficient to allow the commission to make a "prediction as to the result of the construction of these dams."[63]

Here we can see how human actions and impacts are erased, but not completely. The dam, which humans built, would virtually destroy the fisheries, Dr. Higgins had said. No matter how much back-tracking or down-playing he engaged in, he could not fully remove this human figure (in part because the conversation was being recorded and, as such, Higgins had no opportunity to erase what he had previously said; he could only carry on in a new direction). Unsurprisingly, it is difficult to read what he said next without the dam spectre lurking in the background. It coloured what followed to the extent that it is somewhat confusing and jarring to read that the real question does not concern dams at all, but amounts of plankton. The reader expects more discussion on the dam; however, none is forthcoming. Furthermore, we are not sure if this dam is even real or not. Higgins noted that the dam effects *would be* such and such, not that they *are* such and such. Higgins referred to the dam's effects using the conditional tense, which is used to indicate hypothetical or unreal situations in the present or future. Thus, the dam effects may or may not become real. Finally, we do not even know how many dams there are (or are not). Higgins at first referred to *the* hydroelectric dam and then later to *these* dams, but he never specified what the other ones were or even if there were other ones. The effects, their severity, and even the presence of the dam(s) remained unclear. This is not to say that the dam(s) and its/their associated aspects were cleanly excised; they continued to exist in an undecided spectral form that haunted the remainder of Higgins' discussion.

---

[63] Shelford (1934), 496.

In outdoor recreation another interesting haunting occurs. Horses and pack animals are haunted by the spectre and trace of humans. There have been studies conducted on the effects of horses and llamas on vegetation. These effects, it turns out, are quite similar to those of hikers (although there are important differences, as well), ATVs, and bikes. They are not, however, similar to those of native animals, such as deer, moose, or elk. This is because native animals are natural while horses and llamas are not. This begs the question of when a horse is not a horse. The answer: when it is a human. Horses and other pack animals are treated more like people than they are treated like elk and moose. Pack animals are haunted by the human spectre in OR discourse.

In another example from ecology, Eugene Odum, in the 1983 version of his popular textbook, explained the cyclical character of nature by noting that population growth charts show periodic oscillations. The hare and lynx populations in Canada are the classic example. Since about 1800, the Hudson's Bay Company (HBC) has kept records of the number of pelts traded. These records show a remarkable amount of regularity, which was taken to indicate the natural cycle of these populations. Both populations, Odum remarked, clearly peak and decline every nine or ten years.[64] What is significant about this example is what is not mentioned. It is the unmentioned point that is important. Odum claimed to illustrate the natural cycles of hare and lynx populations in Canada. However, he made no mention of the influence the HBC's calculation of population numbers might have had on the population numbers. The HBC was recording pelts, not live animals. The presence of the data themselves is reflective of the presence of humans hunting and trapping in nature. And this presence was hardly minimal. In 1865, the HBC recorded 150 thousand hare trapped and, in 1868, over 70 thousand lynx were trapped. Odum then noted a decline in the hare population two years later, in 1868 (only two thousand trapped), and a similar decline five years later for lynx, in 1873 (down to three thousand). According to Odum, this was a natural cycle.

---

[64] Odum (1983).

However, it was through the HBC's interaction with hare and lynx that we came to learn about population cycles in nature. Our measurement apparatus (the HBC's practice of trapping and recording) cannot be extracted from what it measures: the measurement of the cyclic oscillations forms part of the oscillations themselves. This knowledge has, in turn, influenced the way we interact with nature. The hare and lynx case, for example, has been used extensively in the ecological literature to illustrate population cycles, checks, and balances in nature.[65] This model of population cycles has subsequently been used to structure studies in OR that justify the imposition of levels of use or of restrictions on the grounds that nature cycles naturally, and our interaction with it upsets this pattern. What has been forgotten in this process is the role that humans played at the origin of our knowledge about population cycles. Although forgotten, humans cannot be erased completely. If we look carefully, we can see the ghosts of those trappers and traders in current OR policies regarding the maintenance of natural processes in the wilderness. Acknowledging the spectrality of HBC trappers today does not suggest a solution. Instead, it reminds us of the mystery and inherent 'unsolvability' or undecidability of questions about natural nature.

---

[65] Loo (2006), for example, discusses the impact of Elton's research (beginning in the 1920s) using the HBC records. Through the use of these records, Elton discovered many of the most significant developments in animal ecology. Elton, in fact, is often referred to as the father of animal ecology because the work he did with the HBC records essentially brought animal ecology into existence. The question of periodic fluctuations in animal numbers was first addressed by Elton in 1924, when he suggested that population fluctuations for certain species remain constant regardless of the geographical variations in populations. In other words, what causes these fluctuations must have a nearly global effect and be highly regular in timing. His argument was that periodic sunspot activity could account for the observed fluctuations. See Elton (1924) for the details.

In another example, in 1957 Howard Odum studied Silver Springs in order to construct an energy balance sheet that accounted for all the incoming and outgoing energy. Odum aimed to show how a self-contained ecosystem was balanced from the perspective of energetics. In other words, he wanted to show that all the incoming energy could be accounted for in the functioning of the ecosystem. If the system required more energy than it received, it could not be sustained over time. He concluded that incoming energy compared to outgoing was roughly equal. That is, the amount of sunlight and other materials entering the spring ecosystem equalled the increase in biomass plus downstream outflow of materials plus radiated heat energy. However, in order to construct this balanced energy account, Odum needed to discount the loaves of bread that people were throwing into the stream headwaters to feed the fish. According to Odum, the "only definitively demonstrated … organic input is the 70 loaves of bread (365 gm each) fed to the fish each day. This import is neglected in Table 15,"[66] which otherwise represents a total annual balance sheet for Silver Springs. The separation of humans from nature in this instance could not be achieved cleanly or completely. The loaves of bread made their way into the spring ecosystem but, even though he acknowledged their existence, Odum removed them in his calculation of the functioning of that system because he saw them as artificial importations. However, the loaves of bread may be removed in some sense, but not in others. The final totals he arrives at contain and hide within them those loaves of bread. Human actions are haunting this ecosystem; they are not fully present. The actions of the past haunted Odum's future calculations in a manner similar to the way in which the past actions in *Hamlet* haunt Hamlet in the form of the ghost of his father. To be or not to be is a question Hamlet asks both himself and the ghost of his father, and one might also ask this of the loaves of bread in Odum's study: are they there or not?

---

[66] H. Odum (1957), 106.

Ghosts or spectres, traces and différance, and undecidability are all paradoxical at some level. These presences hover just at the edges of sensibility and comprehensibility. As such, they are difficult to reconcile with a scientific method that privileges logic, reason, and deductive thinking. As ecology and OR sought to understand nature from a more scientific perspective, these presences continued to intrude. In large measure, these fields issue out an attempted separation of humans from nature. Excluding people from natural nature or vice versa creates a hole into which can be read the spectral presence of that which has been ousted. Thus, even in the most natural of landscapes, the human presence is still felt. Furthermore, natural nature is not a passive entity uninvolved in the development of knowledge in ecology and OR. Ecologists and OR researchers were often surprised by what they found in their studies. I contend that this response is partly due to the agency of nature as it acted back on our models and theories. This kicking back, so to speak, of natural nature onto the work of scientists in both disciplines is one element in understanding how natural nature is an actant in the process of real-ising the common world and will be explored further in Chapter Seven.

In terms of the paradoxical character of traces and ghosts, neither OR nor ecology has a robust method for addressing them. Paradoxes, as we will see in the next chapter, offer some difficult challenges to the scientific method and rational thought. In the notion of paradox we can see the trace at work alongside différance and spectres. Traces and spectres, as noted, are somewhat paradoxical and have not been easily incorporated into either ecological or OR discourse. In fact, neither discipline has gone much beyond mentioning that certain concepts are paradoxical. To be sure, paradoxes have been noted by researchers in both ecology and OR; the focus of the next chapter, then, is a further explication of the idea of paradoxes and an illustration of how they can provide another way to think about the nature/culture split and an alternative way by which to produce and arrange knowledge in each discipline.

CHAPTER 6: THE NATURE OF PARADOXES/THE NATURAL PARADOX

Man learns from history that man learns nothing from history. ~ Hegel

Paradoxes are an idea that has appeared repeatedly throughout this work. In Chapter Two, I suggested that the ecological knowledge produced through most of the twentieth century followed the strictures of science and rationality. In so doing, the treatment of paradoxes was relegated to a more minor position. As discussed in Chapter Two, ecologists relied heavily on structural analysis to probe deeper into the secrets of nature. They attempted to identify the main structural components of ecological systems and then to abstract out this structure into a general model for nature (Odum's universal energy model is one example of this). This emphasis on structure has been critiqued by poststructuralists who suggest structure can become self-validating, which, in turn, stifles the animating force of creation. Derrida, in particular, has pointed to some of the limits of a structural analysis. His argument identifies certain paradoxes of structure that can also be seen in the discourses of ecology and outdoor recreation. Even as ecology and OR sought universal standardized terms and concepts, meaning continually disseminated outward in an endless series of slippages. These slippages are also part of the paradox of signification where meaning cannot mean what it means and, instead, traces itself through the excess that deconstructs the text.

In outdoor recreation discourse, the question of change has proven a difficult one to clearly elucidate. Efforts to protect valuable wilderness areas brought OR researchers into contact with the question of which changes were acceptable and which were not. Many different answers to this question have been offered, and all of them have their detractors and/or problems. We saw, in Chapter Three, that many of these attempts mired

OR researchers in a paradox. In addition, the atomic self to which OR benefits accrue is itself a product of a modern scientific view. This viewpoint privileges and produces knowledge that is factual, reliable, verifiable, and generalizable. Knowledge of this sort is difficult to reconcile with paradoxes because paradoxes are not necessarily factual, reliable, verifiable, or generalizable. It is much more difficult to make recommendations and policies based on paradoxical knowledge or conclusions. Nevertheless, in the benefits literature, paradoxes are present. This chapter explores the potential paradoxes have for decentring scientific knowledge. When paradoxes are explored, and even invited, previously foreclosed options become available.

Given the centrality of this concept of paradox, it seems appropriate to investigate further the meaning and power of paradoxes. This is the goal of this chapter. I wish to explore the idea of paradoxes and their potential for addressing ethical and intellectual challenges in ecology and OR. Dealing differently with paradoxes is one step ecologists and OR scholars could take in order to develop an alternative option to the scientific method. A further step in this process involves the perspective of science studies, a position associated with Bruno Latour. This chapter, then, ends by outlining how science studies contributes to the understanding and handling of paradoxes in a different manner than the modern scientific method does.

Theory of Paradoxes

> *Paradox is a pointer telling you to look beyond it. If paradoxes bother you, that betrays your deep desire for absolutes. The relativist treats a paradox merely as interesting, perhaps amusing or even, dreadful thought, educational.*
> *~ Frank Herbert*[1]

A paradox is a difficult discursive entity to discuss. Often times it seems to be used synonymously (and incorrectly) with irony. Irony results when the meaning of a statement is not what it appears to be at first. When we say one thing and really mean something else, often the opposite of what we say, we are being ironic. Irony, while surprising, is comprehensible; paradoxes, on the other hand, have a more difficult relationship to logic and understanding. Irony is the unexpected; paradox is (seemingly) self-contradictory.

Most of the time, when a paradox is noted in ecology or outdoor recreation it is actually a case of irony. For example, it is actually ironic, not paradoxical, that "the more knowledge we acquire [about nature], the more uncertainty we encounter—which renders planning for conservation a sticky business indeed."[2] One would expect the reverse: as we learn more, we gain in certainty. In OR circles, the most common manifestation of a paradox is the 'loved to death' paradox, which James Turner formulated in the following manner: "[a] paradox underlay the newly-established wilderness system. How could these areas be made available for public use with minimal restrictions, while also preserved as a resource for posterity?"[3] Many scholars have called this the 'paradox of wilderness,' and it forms a significant part of outdoor recreation discourse.[4] Essentially, the argument claims, outdoor enthusiasts, because they are attracted to wilderness areas in ever-greater numbers, destroy the very thing they love. This argument, too, is ironic rather than paradoxical: it is ironic that loving wilderness results in the destruction of it. In spite of the confusion between paradoxes and irony, there is a place for an analysis of irony in OR. Irony is quite helpful because it points to situations where intentions may not match the outcomes of actions. When the irony in loving wilderness to death, for example, escapes notice, it is harder to argue for changes in people's behaviour, because the effects of their actions appear to be in accordance with the intended result. Nonetheless, there are important paradoxes that are different from ironic situations. In order to understand paradoxes, we need to understand paradoxes in general. This proves, however, to be no simple task.

---

[1] Hebert (1981), 277.

[2] Lister and Kay (2000), 189.

[3] Turner (2002), 463-484.

[4] Miles (1985) claimed that "[t]here is a threat to wilderness, and it is simply that wild country in the United States is being loved to death," 96. For other examples of this paradox see Butler (1989), Carter (1997),

Paradoxes are defined in the Oxford English Dictionary (OED) as "apparently absurd or self-contradictory statements or positions or strongly counter-intuitive ones that nevertheless prove, upon analysis, investigation, or explanation, to be well-founded or true," "a statement that is (taken to be) actually self-contradictory, absurd, or unreasonable," or "an argument that uses (apparently) acceptable premises and (apparently) valid reasoning to arrive at a conclusion that is against sense, logic, or acceptability."[5] At first, this definition of paradoxes seems to make sense: paradoxes are statements that are contrary to what we expect or believe.[6] However, there must be more because otherwise paradoxes are indistinguishable from irony. The OED definition

---

Hammitt and Cole (1987), Kuss and Graefe (1985), McGivney (1998 and 2003), More (2002b), Sacklin (1998), Shogan (1990), Strathcona County Parks and Recreation (1987), Wall (1989), Wilkinson (1992), and Woods Gordon Management Consultants, Richard A Nuxoll Consulting Services Ltd., and MTB Consulting Ltd. (1985).

[5] Oxford English Dictionary online, accessed April 7, 2007.

[6] The 'paradox' of wilderness mentioned above is an example of this aspect of paradoxes: loving wilderness produces results that are contrary to what we expect.

argues that paradoxes are seemingly or apparently self-contradictory. If they are only *seemingly* self-contradictory then they are not *really* self-contradictory. Paradoxes, then, are not simply contrary to what we expect (for this is irony), nor are they *actually* self-contradictory (for they turn out, upon further investigation, to be true). The OED definition is particularly interesting for its use of parentheses and offers a possible avenue for understanding paradoxes. If a paradox was either self-contradictory or not, there would be no need of parentheses. So, how should these parentheses be read? Paradoxes are not self-contradictory nor are they straightforward statements. They are not ironic, nor are they declarative. What are they then? Do paradoxes exist or not? If they exist, why are they so hard to define? Explanations of paradoxes, it seems, invoke them. In this sense, the text says more than it means, more than it is capable of meaning. Its performance is at odds with itself. Thus, paradoxes have a complex relationship with truth and logic: they can be both true and false, or neither true nor false.

Perhaps some examples will help. Probably the most famous paradox is the Liar's Paradox: 'This sentence is false.' The Liar's Paradox violates the principle of bivalence that says statements must be either true or false. Logicians claim that the only way to understand this paradox is to see it as neither true nor false. The concepts do not apply to it. This understanding does appear to resolve the quandary; however, what does it mean that the Liar's Paradox is neither true nor false? Does the sentence have any meaning? Does it have too much meaning? How are we to understand it? More difficult than the original Liar's Paradox is the refined version: 'This statement is not true.' Whereas the Liar's Paradox can only be understood as neither true nor false, the refined version cannot. If the refined version is not true, then it states an accurate proposition and becomes true. But if it is correct, then it must be, as it claims, not true, which is to say, true. The refined Liar's Paradox must be understood as both true and false.

The meaning of a paradox is not fixed: following its 'logic' continually results in following its 'logic.' Take for example 'This statement is not provable.' If we can prove it, it states an accurate condition; thus, it is not false. And if it states an accurate assertion, it is, as it states, not provable, except that we just proved it. On the other hand, if we cannot prove it, it becomes true; only we cannot prove it. This loop continues endlessly. The paradox of the barber illustrates this movement even more clearly. Suppose there is only one barber in town. All the men in town who do not shave themselves go to this barber. Thus, the barber shaves all and only those men who do not shave themselves. Thus far, there are no problems. But who shaves the barber? If he shaves himself, he cannot be shaved by the barber, which is he. Therefore, he must not shave himself and go instead to the barber, which is he and, thus, he shaves himself, so he must not go to the barber, which is he, so he must shave himself, and so on and so on. There is no end to this line of logic because it is not a line; it is a circle. In other words, paradoxes are undecidable and non-fixable, and that is why we must decide and fix them.[7]

---

[7] The degree to which this sentence does not make sense indicates the troubling nature of paradoxes that I am speaking of here and, thus, it now makes sense. This means we should take paradoxes as they stand without trying to re-word them to make sense: to do so only loses their meaning. Yet, as they stand they are illogical and make no sense and, thus, are paradoxical, which proves my point.

(Post)structural Paradoxes

They turned together, turned in a direction that wasn't usually there.
~ Neil Gaiman[8]

In three years from now, the Internet bubble will have burst ten years ago.
~ The Daily Show with Jon Stewart

Some theorists, mainly poststructural ones, have encountered paradoxes while theorizing about language, meaning, and signification. Language is a tool, yes, but not a perfect one. For poststructuralists, language does not arise from consciousness. Instead, they affirm the power of language on consciousness: consciousness is the product of the meanings and images humans recognise and engage with. One thing that language cannot be made to do, then, is engage the question of what lies outside language and thought without enveloping itself in a paradox. How can language refer to the time before thought? To do so requires the use the very tool to ask and describe what the situation would be like without that tool. This conundrum is Derrida's observation on Foucault's discussion of *Cogito* and the origin of rational consciousness. According to Derrida, we cannot discern the moment of the emergence of the *Cogito* from what lay before it by using the techniques of the *Cogito*. However, when we remain within the western metaphysical tradition[9] we have no other option but the techniques of *Cogito*.[10] Paradoxically, Derrida's response to Foucault (knowingly) explained the origin of language as unexplainable. The more accurately Derrida captured this situation, the more he could argue he had shown the origin of rational consciousness. Of course, the more accurately he did, the more inexplicable it became.

---

[8] Gaiman (2005), 366.

[9] This "metaphysics encompasses any Western European conceptual system that opposes the earthly to the transcendent, the real to the ideal, the world to a text, the referent to its sign, an original to its representation, a content to its form" (Bracken 1997, 26).

[10] Derrida (1978a).

The beginnings of rational thought and language are not the only place that one encounters linguistic paradoxes; one can no more think about the beyond of language's limits than one can about its origins. 'What is it that language cannot describe?' is a question that cannot answer in any simple fashion. These

> problems are the traditional problems of any philosophy which leads knowing to a knowledge of ultimate knowability, and thereupon summons knowing to un-know itself.... Unfortunately for such philosophies, unknowing is something that only happens to us from the outside; it can never be a *consequence* of knowing. We can no more know ourselves into un-knowing than we can consciously make ourselves go to sleep or consciously make ourselves cease to remember a piece of information.[11]

Language and knowledge can never be called upon to describe their limits with finality or totality. This paradoxical situation occurs in ecology and OR when researchers attempt to describe nature as outside the limits of culture: how can researchers, or anyone for that matter, know what nature is as long as it lies outside human culture? This situation mirrors the one Derrida encountered when explaining the origin of the *Cogito*.

Language also displays a limit in its ability to describe the world, an indescribable limit. As Catherine Belsey remarked, if words named things that existed outside language, there would be exact equivalents from one language to another. But the assumption of verisimilitude in the way words relate to the world does not follow. There are instances where words in one language are not easily translated across into other languages. And some words in one language do not exist in others. As a result, "we are compelled to conclude either that some languages misrepresent the way things are, while our own describes the world accurately, or that language, which seems to name units given in nature, does not in practice depend on reference to things, or even to our ideas of things."[12] If words do not name things that exist outside language, then we never actually speak directly about that world. This inability has made numerous environmental philosophers, environmental historians, and OR scholars sceptical of poststructural theory because of its unspeakable threat of a vanquished external world.

---

[11] Harland (1988), 118.

Paradoxical questions and problems begin to illustrate the structure of language.[13] At least in western culture, most, if not all, languages claim to depend on reason, rationality, and logic. These languages claim they are comprehensible. Comprehensibility is, in fact, the distinction we make between madness and sanity. People who make sense are not insane. This distinction between the sane and the insane is what founds the *Cogito*, the thinking subject, and its emergence marks the distinction between absolute madness and reasoned sanity. As Derrida suggested, the moment we thought the totality of knowledge, the origin point of reason and non-reason, is the moment we escaped this moment. This escape is the original moment of *Cogito*. And it is profoundly unsettling, for to escape this totality we flee either toward infinity or nothingness.[14] When reason unites with non-reason, everything and anything becomes allowed and nothing is prohibited: both the logical and illogical exist side by side—actually, these two terms are not appropriate here as the moment under discussion is one *before* the splitting off *Cogito* (rationality) from madness occurred. The western metaphysical tradition has no words or concepts for this moment because language is itself a product of that split and came after that moment.

---

[12] Belsey (2002), 9.

[13] I do not claim that all languages operate in this manner; however, I do suggest that both ecology and outdoor recreation are products of the type of linguistic requirements and structure I describe here.

[14] Derrida (1978a). This is similar to one of the paradoxes that Epicurus wrote: "[h]e who says that all things happen of necessity cannot criticise another who says that not all things happen of necessity. For he has to admit that this assertion also happens of necessity" (quoted in Grove 1989, ftn. 71, 199). In Derrida's case, we might re-word this to say: he who says all things happen rationally cannot criticize another who says that not all things happen rationally. For he must admit that this assertion is itself rational. This paradox is interesting, as we shall see, because it reflects the basic paradox of more recent ecological theory, which may be one reason that OR has yet to adopt any of this literature.

These poststructural paradoxes offer important insights into the workings and limits of language. Ecology and outdoor recreation have linguistic components and are, thus, implicated in the same linguistic functions and limits just outlined. Furthermore, poststructuralism's response to paradoxes engages them more and allows the playing movement of différance and traces to operate. Paradoxes are difficult in large measure because this playing movement makes meaning slippery. It is hard to pin paradoxes down to a stable meaning. Far from being a detracting feature of paradoxes, I contend that their movement should serve to remind us of the inherent slippage of meaning that is always occurring. It is, no doubt, difficult to comprehend this movement. As a result, I suggest that ecology and OR should take their time with paradoxes and explore their meaning(s) more thoroughly.

Structure as Object: The Paradox of Structure and Structuralism

> *... the abstract math that's banished superstition and ignorance and unreason and birthed the modern world is also the abstract math that is shot through with unreason and paradox and conundrum ... ~ David Foster Wallace*[15]

Nearly all ecologists, as well as OR scholars writing about recreational impacts, are structuralists, whether they use this label or not. As noted in Chapter Two, ecologists used structure as a powerful metaphor to examine the deeper, more fundamental aspects of nature. Likewise in OR, structure played a very important role. Wilderness was seen to have a particular structure that made those spaces valuable. On the one hand, researchers spent much time and energy looking for ways to interact with wilderness without altering its original structure. On the other hand, they argued for interventions, whenever that structure had been altered, in order to build it back to what they perceived as the proper configuration. In both disciplines, then, structure plays an important role.

---

[15] Wallace (2003), 30.

The significance of structure is matched its near invisibility vis-à-vis analysis. Ecologists and OR scholars used structure as a tool to analyze nature; they did not analyse the nature of those structures. Consequently, structure was used non-reflexively, as if it were a perfect tool that could represent situations as they actually were and that did not take the detour through signification. Although this point appears highly critical of structure, we must remember that Derrida's

> object is not to deny or invalidate the structuralist project but to show how its deepest implications lead on to a questioning of method more extreme and unsettling than these thinkers wish to admit. The very notion of 'structure' is shown to be a metaphor dependent, at the limit, on a willed forgetting of its own rhetorical status. Two of Derrida's most powerful essays—'Force and Signification' and 'Structure, Sign and Play'—are devoted to precisely this end of bringing out the radical metaphoricity of 'structure' as a term and an operative concept.[16]

The poststructural challenge that Derrida and others raised toward structural analyses in literary theory, anthropology, linguistics, and philosophy (to name only some) is germane to both ecology and OR as they are also structural projects. Structural analysis in ecology, for instance, is not wrong or misguided. Rather, if one follows its logic and implications through, structural analysis actually leads to a more extreme and unsettling place than originally thought. In the next chapter I follow the logic and implications of structural analysis into that unsettling territory as a means of incorporating poststructural theory in ecology and OR. Before exploring those implications, I use structure as a means of pointing to certain paradoxes in ecology and OR. I do so not to invalidate structural analysis. I wish, actually, to extend its power by moving it into poststructural theory.[17]

---

[16] Norris (1998), 79.

In 1961, Rowe examined the various classification systems and structures of knowledge that ecologists had developed over the course of the century. That is, he discussed the structure of structural analyses in ecology. It is interesting to notice that the most common element in all the classification structures is the reliance on visual metaphors. All classification systems, including the ecosystem concept, relied on visual metaphors. For example, Rowe, explaining Woodger's theory, said that

> [a]n object is what a slab of space-time is known as, and to our perception it is basically a volume, a form with related qualities. Thus the first viewpoint is morphology, the apprehension of physiognomy or form. Beyond perception of form, understanding is added in two ways. One mode of comprehension is analytical, looking within to the morphology of parts in their structural relationships (anatomy) and perhaps making an inventory of them (composition). The other mode of comprehension looks outward, seeing the object whole and searching for its relationships with the external world.[18]

Rowe's reliance on the visual aspects is overwhelming. Ecologists look within, look without, and see and perceive form, volume, physiognomy, objects, slabs, and qualities. Geometrics are part of the structure of the way ecology structures its own knowledge. Regardless of what system Rowe discussed, the structures used were spatial and visual. Nature was diagrammed as a machine in some instances. In others, it was shown as a thermodynamic system. In others still, it was depicted as a super-organism. These structures were all pictorially rendered and made visible. This is Derrida's comment about structuralism in general: it relies heavily on visual geometry at the expense of animating force. Structural analysis, according to Derrida, exceeds encapsulating the totality of content by excavating the form of that content and focussing on its structure. This structure is content made visible; it is form, shape, relations, and arrangements.[19] Ecologists and OR scholars likewise privilege visual form in their structural analyses.

---

[17] In this sense, the term poststructuralism is a poor choice as it connotes a certain antagonism toward structuralism. I use the term here with an understanding of the complimentary nature of structuralism and poststructuralism.

[18] Rowe (1961), 423.

This privilege is certainly true in Rowe's analysis of the different levels at which one might study nature (Figure 4.1[20]). His analysis relied heavily on visual metaphors. At all levels, he diagrammed nature and showed particular types of relations with other levels that were themselves placed in spatially significant arrangements (that is, above or below). This diagram is

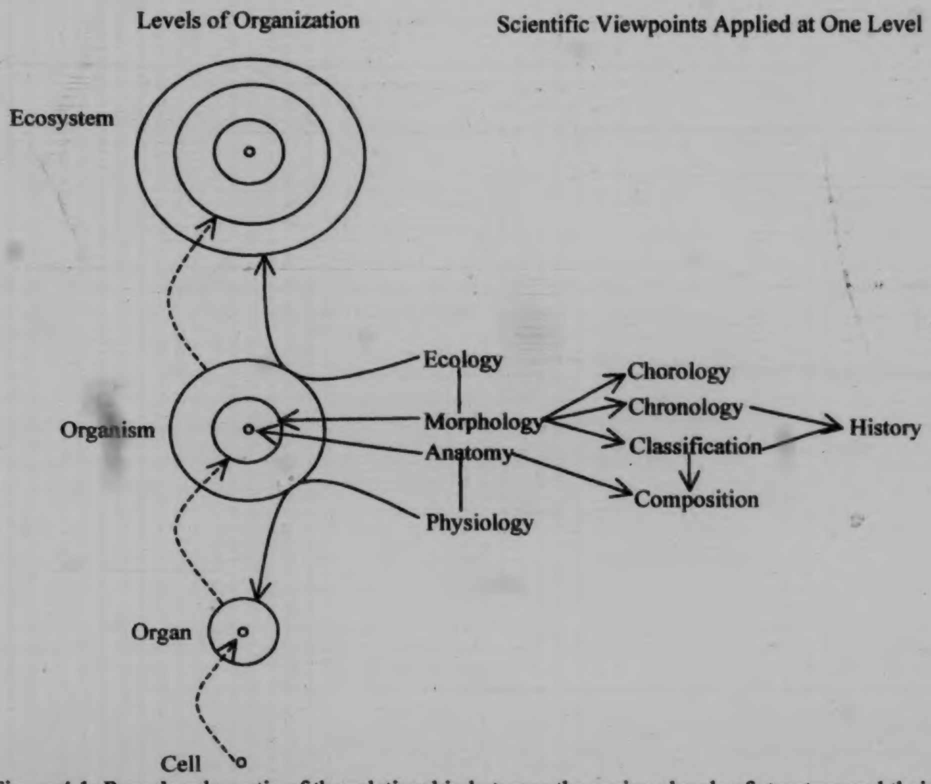
Levels of Organization
Scientific Viewpoints Applied at One Level
Ecosystem
Organism
Organ
Cell
Ecology
Morphology
Anatomy
Physiology
Chorology
Chronology
Classification
Composition
History

Figure 4.1: Rowe's schematic of the relationship between the various levels of structure and their corresponding type of analysis.

---

[19] Derrida (1978b).
[20] Rowe (1961), 425.

an example of the power of structural analysis. The diagram illustrates the relationships between the levels of nature as well as the type of analysis relevant for each level. With the use of this diagram, Rowe was able to show how each level is related to the one above and below. He could illustrate how certain types of analysis are appropriate for certain levels of organisation. For example, physiology is useful in studying the organism. However, to really understand an organism, the physiology of its constituent organs must also be examined. Thus, physiology starts at one level and works down. Ecology, on the other hand, would begin by studying an organism: its requirements, its abilities, its behaviour, and so forth. But, in order to really understand an organism, ecologists must also understand how it fits into its environment. Thus, ecology must also move up the hierarchy and study the organism in its environs.

The size of the circles is also important. Each level nests neatly in the level above. The graphic depiction informs ecologists that organisms live in ecosystems, that organs are contained in organisms, and that organs are composed of cells: as the saying goes, 'everything in its place and a place for everything.' The relations in this diagram, as with nature, are physical: what interacts with what, what is inside what, what touches what, what eats what, and so forth. This graphic pattern is repeated on up the hierarchy of levels of integration.

The mirroring of physical and spatial relations in the diagram makes this particular structural analysis all the more powerful. Rowe suggested that through this type of logical analysis "we obtain a system of nature, a register of the whole."[21] From this observation, Rowe drew the conclusion that "the structural similarities and logical homologies of the many different subordinate and superordinate systems make possible the formulation and deduction of principles valid for systems in general, giving rise to a new basic scientific discipline: General Systems Theory."[22] Because all systems are thought to operate on similar principles, not only can an ecosystem be analysed in terms of its overall structure, but so too can a series of nested systems from the smallest (a cell) to largest (the universe). In other words, the total structure of nature is opened up to analysis and understanding. A particularly ubiquitous example of structural diagrams is Odum's universal energy model that depicts the structure of any system in nature larger than an individual animal. For Odum, natural systems are best understood as flows of energy, and the spatial arrangement of the components relays important information beyond what can be determined by the components. For example, Odum's diagram shows how decomposers function and are related to other elements in the system. The particular species or individual decomposers are not important or even known.[23]

---

[21] Rowe (1961), 425.

As these diagrams show, structural analysis is visual. It focuses on form. It renders functions in visual relations through schematics. Visual structures explain much about nature; however, they explain only certain aspects and, in the process, hide others. A variety of analyses help give a more complex understanding of natural systems, except that some analyses cannot be conducted at the same time as others because the setting of one experimental framework precludes the possibility of certain alternative ones. Odum's universal energy model is an example of how one type of analysis precludes others. When his diagram represents the *universal* structure of (eco)systems, the specific content must be left out. In other words, the diagram cannot be both universal and particular at the same time. Even when a particular ecosystem is used to illustrate the workings of all systems, any particulars that do not already have a conceptual category are left out. Decomposers, for example, are included, but only as per the diagram's instructions on what their role in the system should be. A list of the specific decomposers might be present, but anything not already in the model cannot be made to appear. In another example, Eugene's brother, Howard Odum, conducted an early energetics study that calculated the total energy balance sheet for the Silver Springs ecosystem.[24] In addition to the seventy loaves of bread per day that he discounted, he also discounted the bubbles on certain plants in the spring. The bubbles were measured, but discarded as insignificant as there was no relevant category for bubbles in his energy analysis of the structure of the spring. The problem is not that spatial models are wrong; they are a necessary and powerfully explanatory tool. The problem arises when we think they represent a complete explanation of nature.

---

[22] Rowe (1961), 421.

[23] Odum (1993), 97. See Chapter Two for more discussion of this model.

We can also see another troubling aspect with the conceptualization of structure in ecology and OR. Both disciplines often assume that a system is spatially closed. That is, a stream or forest has boundaries that completely contain the system in question. This is evidenced in Rowe's use of circles. Each level of integration becomes a self-contained entity with clear boundaries. The geometrics of this figure artificially accentuate the clarity of the boundaries: where exactly does the forest end? Another ecologist, O'Neill, criticised the ecosystem concept precisely because it requires such solid boundaries. He suggested that the spatial closure assumed within the concept is one of its leading detractions.[25] This problem, however, remains hidden, for the most part, in the illuminating light of structure.

---

[24] Odum (1957).
[25] O'Neill (2001).

Structure hides by illuminating. The near omnipresence of geometric metaphors combined with their power to depict without appearing to signify makes their use unreflexive. The fact that Rowe used visual metaphors, the most common element in all ecological classification systems, to describe the similarities in all ecological classification systems but never mentioned visual metaphors as another similarity is telling. In making so much visible, structure is able to hide itself from analysis, especially in structural analyses. It hides through illumination: it is more difficult to see something the closer it is to the light bulb of truth. When spatial metaphors seem linked *of necessity and self-apparently* to that which they depict, they become invisible transmitters of information. Rowe's structure seems not to speak of nature; it does not even appear to speak for nature: it is mute and in that silence nature just appears.

Due to the invisibility of spatial metaphors, it is difficult to examine the role of the structure in creating knowledge. In the case of outdoor recreation, scholars have focussed on determining, preserving, and/or recapturing the authenticity of wilderness. The meanings visible within this structure became important, but not the meaning of the structure. According to Derrida, structure is itself a form; it is geographical and visual. However, Derrida claims, "the relief and design of structures appear more clearly when content, which is the living energy of meaning, is neutralized."[26] When OR scholars or ecologists neutralize meaning and content, the essential form of the structure—its components, its layers, its relationships and interrelationships, and its hierarchies—is retained. However, it is exactly the content and meaning that the structure hopes to explain. This contradiction points to the paradox of structure. The notion of structure is at some level self-contradictory, but it is also obviously manifest and powerful.

---

[26] Derrida (1978b), 5.

Derrida claims that the weakness of structuralism lies in this very treatment of meaning. Structure tempts us to see meaning as if it were inside structure, inside the pure language of structure (that is, a non-metaphorical language) that operates at no distance, only perfect proximity. We do this, for example, when we think, without ever really realising it, that nature is a nested set of systems that relate to each other according to certain functions. That we cannot locate meaning completely inside a pure structure motivates the anxiety that is hidden by the illuminating power of structure. Even in structural analyses that claim to give equal weight to form and force, to meaning and structure, Derrida finds that inevitably they do not. Structure privileges spatial models. Even when one sees the interplay between space and time, time often becomes reduced to a single element in which form can be displayed. Time is the agent for a structure to stand as such. Time calls for measurement because it is always accompanied by a line and is always extending in space. The over-emphasis on geometry is corrected by a mechanics of movement (shifting content), not by an energetics (force or creation). Within structural analyses of this type, authors explain "everything in nature with figures and movements, and of ignoring force by confusing it with the quantity of movement."[27]

Geometric structure speaks of direction, development, ascension, but not force of movement. The structure is pre-potent and transmits its genetics through time (which has become another vehicle for spatiality) to future geometric manifestations. For Derrida,

> [s]tructuralism above all insists upon preserving the coherence and completion of each totality at its own level. In a given configuration it first prohibits the consideration of that which is incomplete or missing, everything that would make the configuration appear to be a blind anticipation of, or mysterious deviation from, an orthogenesis whose own conceptual basis would have to be a *telos* or an ideal norm. To be a structuralist is first to concentrate on the organization of meaning, on the

---

[27] Derrida (1978b), 16.

> autonomy and idiosyncratic balance, the completion of each moment, each form; and it is to refuse to relegate everything that is not comprehensible as an ideal type to the status of aberrational accident.[28]

However, structuralism cannot actually be true to its promise. For as much as it claims to reject the idea that structures are whole and orthogenic, it cannot in its actual practice. "Whether biology, linguistics, or literature is in question, how can an organized totality be perceived without reference to its end, or without presuming to know its end, at least?"[29] This question leads to one of the insights poststructuralism has for structuralism: the search for totalities, for a total field of representation in which everything can be accounted for is subject to necessary failures. Instead of this total field, this complete structure, researchers should be searching for the paradoxical playing movement that underlies all signification systems.

Derrida also reminds us that structure is not objectively present. Structure is not out there somewhere awaiting the insightful analyses of structural anthropology, literary theory, philosophy, ecology, or outdoor recreation. Consequently, structure is not objectively present any more than are the other things one encounters in nature. The limitation of structuralism is partly that it takes structure too seriously. There is a tendency to pour all effort into making elaborate and intricate diagrams of structure that capture the totality of its subject matter. Poststructuralism offers the reminder that structure, while powerful, is neither necessary not totalizing. Ecology and OR, then, should not take the structure of nature as seriously as they have been in the past. It may be as or even more important in some ways; but, it also will be less significant in others. One area that structure remains blind to is the explanation of how certain forms came to exist. Derrida remarked that form fascinates when force does not.[30] The structural analysis of nature is certainly a powerful tool, but it is blind to its own origin. Structure gives the impression that nature just *is* comprised of those elements in those relations. Structural analysis forgets the process of making nature into that structure. Poststructuralism becomes helpful in structural analysis because it reminds us of the role signification plays in the process. Poststructuralism also highlights the paradoxes of structure that are a necessary accompaniment of structuralism.

---

[28] Derrida (1978b), 26.
[29] Derrida (1978b), 26.

These paradoxes can be particularly unsettling to science. Scientific language tends away from paradoxes at the same time as it performs them. Paradoxes are particularly unsettling in science because they cannot be pinned down or fixed. What exactly do they mean? What are we to do with them? How can we resolve them to find answers to pressing questions? Our best problem-solving tools (logic and rationality) are not just ineffective here; they are implicated in the very problem they are being applied to in an effort to solve. And yet, paradoxes do get resolved all the time; language settles itself on the shores of our civilization and roots itself into the soil, standing fixed for all to see. This rooting is a necessary feature of language for it to be useful. It is not, however, inevitable in all respects. Even if this rooting is as necessary as language's failures and paradox, there are choices when it comes to deciding paradoxes and setting down the roots of language. As we shall see, it is this element of choice that ecology and OR could more fully acknowledge. When scholars shy away from the paradoxes inherent in science, and language in general, and unconsciously fix meaning, they escape the moral responsibility involved in making decisions. The ethical response to paradoxes, I suggest, is to alter the way we root them. And one step in this is to comprehend structure differently. By showing the deconstruction of structure, its structurality in other words, we can solicit structure. Derrida suggests that, by using solicitation, structural closure can be seen more clearly: with solicitation, he says, "structure then can be *methodically* threatened in order to be comprehended more clearly and to reveal not only its supports but also that secret place in which it is neither construction nor ruin but lability [a readiness for change]."[31] The solicitation of the (geometric) structure of nature can make ecology and OR more ready for change. The goal of lability is, in fact, what the current chapter is attempting to achieve for ecology and OR. If we assume that structural analysis in ecology and OR is more open to change, we can explore in more detail the particulars of alternative analyses.

---

[30] This is similar to Nietzsche's (1967 [1901]) argument "that one should value more than truth the force that forms, simplifies, shapes, invents" (326).

Science Studies

A sudden, bold, and unexpected question doth many times surprise a man and lay him open. ~ Francis Bacon, 'Of Cunning'

Poststructuralism's contribution to our understanding of the discursive practices of ecology and OR does not mean that its perspective is the only or best one. There are many ways of understanding the nature of reality: at one extreme lies "the Cartesian paradigm of use [that] affirms that human beings are so many discrete subjects whose autonomy is manifest in the choices they make concerning efficient use of the intrinsically neutral objects fashioned for their convenience."[32] At the other extreme lies the subjective relativism that poststructuralism has been criticized for engendering. Although it is unfair to claim, as some critics do, that poststructuralism necessarily denies existence to reality, it is fair to suggest that it denies the existence of the absolute reality to which modernity is closely associated. Reality plays a different role in poststructural theory from the one it plays in the modern scientific paradigm. I have found poststructuralism extremely helpful and enlightening; nevertheless, I find wanting a portion of the response that it presents. I argue, instead, that reality (although not the same reality of modernism) is more of an active player in the process of creating knowledge than is usually the case for many poststructural theories. As detailed in Chapter One, this insight came from explicitly wondering and theorizing about the place and role that nonhumans have in crafting the world.

---

[31] Derrida (1978b), 6.
[32] Kaufman-Osborn (1997), 3.

Bruno Latour has posited a detailed process (much of which I borrow here) for how it is that things come into being in our collective world. This process comes from a perspective called science studies that tries to incorporate the process of scientific discovery with the poststructural understanding that the world is not just simply there. It is a process that more explicitly addresses the issue of objects or nonhumans or material entities or non-discursive/extradiscursive elements, however one wishes to phrase it for now (we will come to a decision on this shortly). The point of departure into science studies begins with the idea of surprise. When a scientist makes a discovery, there is often an element of surprise, which poststructural theory does not account for very well.[33] We have to ask whether surprise can be completely accounted for as an exercise of power and discourse. Certainly both these elements are central: the experiment could not be designed in isolation from the discourse of science (among others). Furthermore, its results, to be meaningful, must arise from relevant signification practices, and the ability to propagate these results is connected closely with the circuits of power, including, but not limited to, gender relations, a hierarchy of disciplines, the persuasiveness of logic over emotion, and an economic structure that allows some projects access to more funds than others. Some scientists, too, recognize much of this limit to poststructural insights. Grove commented that "in an important sense we make reality; but it would be wrong to assume from this that reality is *merely* socially constructed as many contemporary philosophers and sociologists of science contend. For there are at least intimations that there is something 'out there' that leads us to impose order on nature."[34] Although no understanding of any scientific experiment, model, or concept is complete without taking account of poststructuralist insights, still, we should ask whether poststructuralism accounts for the sum total of what happens.

---

[33] Keith Jenkins, one historian who has adopted many poststructural insights, illustrates the diminished role of surprise when he argues that "no historian or anyone else acting as if they were a historian ever returns from his or her trip to 'the past' without precisely the historicisation they wanted to get; no one ever comes back surprised or empty-handed from that destination" (Jenkins 2003, 11). To say that no one is ever surprised is simply not the case: historians are constantly surprised by what they find in the archives or by what interviews uncover. Scientists are also often surprised by what happens in their studies. There is a large body of work in ecology that explicitly mentions how surprise played a key role in grabbing scientists' attention and raising awareness of the cause(s) of the surprise. Surprise, in contrast to what Jenkins said, is an important aspect in all research. The problem begins for Jenkins, I suspect, with the poststructural idea that reality is textual. Jenkins makes the claim, in effect, that because we write the text, the history of that text cannot surprise us. Oddly enough, Derrida (who is often credited with the idea of

Surprising discoveries, I suggest, come about because there is more than just text, imagination, virtuality, or simulacra at work in science. Certainly, for Foucault at least, discourse has its material side.[35] However, the process I am outlining sees that material side as more active than he did. I am interested in the contributions entities other than human beings bring to the process of real-*ising* the world. Of course power has material effects, but do materials have effects that are powerful? The point of entry into this for

---

textuality) comments that the act of writing is highly dangerous and unsettling precisely because of its transgressive nature—we are always surprised that what we wrote jumps far beyond what we meant. This is why historians and scientists are surprised by the text they author as well as the 'facts' they find.

[34] Grove (1989), 6.

[35] See Chapter One.

me was the surprise, often indicated by scientists, that challenges poststructural theory to more fully incorporate material entities into the process of gaining new knowledge.[36] As Latour says: "[w]e are thus going to associate the notion of external reality *with surprises and events*, rather than with the simple 'being-there'… of *matters of fact*."[37] Latour uses the word 'entity' to describe those aspects of real-*ising* the world that are not simply discursive. I am going to suggest another term, one that Kaufman-Osborn uses: artefact.[38] This concept, more clearly than 'entity,' captures the process of real-*ising* such that extricating which portion belongs to human discursive meaning and which to the external, non-discursive matter becomes impossible. Artefacts are made, no doubt, but they are also made of something. Locating the line where these two separate is a mythic search: artefacts are solutions, not mixtures.

One of the examples that Latour gives is instructive here. In the 1850s, when Louis Pasteur was experimenting with what came to be known as lactic acid ferment yeast, he reached a point where experiments in the laboratory generated a somewhat consistent pattern of surprising outcomes. As Pasteur conducted more experiments, he subjected this pattern of outcomes to a number of trials. As the trials continued, the list of attributes displayed grew in length and detail; yet, it still did not pertain to a definitive artefact with a name and a self-consistent identity. "At this point in the text, the entity [artefact] is so fragile, its *envelope* so indeterminate, that Pasteur notes with surprise its ability to travel," that is, that he can pick it up and move it with little loss in cohesion.[39] The surprise noted by Pasteur occurred early in this sequence. It was surprise that triggered more investigation. It is also surprise that often changes the direction of experiment and research, and that forces scientists to account for something outside their current explanatory frameworks.[40]

---

[36] Grove also noted this: "much more important is the fact that science, like life in general, is full of surprises" (Grove 1989, 7).

[37] Latour (2004), 79.

[38] Kaufman-Osborn (1997).

[39] Latour (1999), 119.

In line with poststructural theory, artefacts are not autonomous, pre-existing things awaiting discovery. Instead, the researcher and the artefact are co-created as the process continues. Early on, the artefact has very little existence: perhaps it is merely a strange and tenuous pattern that cannot yet be accounted for. By the end of the sequence, however, the artefact will have emerged as a full-blown participant in our world. This participant is more than the text of the researcher, more than a discursive element in the fields of ecology and OR; it is now somewhat autonomous from the humans involved in this process. It has its own existence. So, we must talk about relative not absolute existence. Existence shifts from textual existence to a more autonomous existence as the process of investigation continues. Surprise is the motivator here; it can shift the kind of existence an artefact has. Now, existence can be spoken of in terms of textual reality and autonomous reality (except these two can never stand completely alone—what is external reality with out some way of making sense of it?). When I speak of artefacts, then, I am including both textually and autonomously real elements inseparably combined. The relative amounts of these two types of existence, however, do not have to remain constant: artefacts are solutions, yes, but they come in different concentrations.

---

[40] A note is perhaps warranted here. The explanatory frameworks scientists bring to experimental settings arise within particular discursive structures that shape not only how the scientist thinks about future experiments, but also the physical set-up of the laboratory and the practices carried out within it. So, to suggest scientists come to experimental settings with clean slates, with their biases suspended, or without any preconceived ideas ignores the powerful forces of discourse that surround and permeate the experience. However, what I propose here does not deny the influence of the factors just outlined. Discourse may indeed shape thinking and laboratory practices, but there still can be other players involved in the process of creating knowledge.

According to Latour, under our current understanding or paradigm (what he calls the old settlement or old collective), we cannot see alternative types of existence because an artefact either exists or it does not:

> [b]y asking an entity to exist—or more exactly to have existed—either nowhere and never, or always and everywhere, the old settlement limits historicity to subjects and bans it for nonhumans. And yet existing somewhat, having a little reality, occupying a definitive place and time, having predecessors and successors, these are the typical ways of delimiting what I will call the *spatiotemporal envelope* of propositions.[41]

When this envelope expands, the artefact gains reality and autonomy. We might say that it enters into a wider discursive field where it is not subject to the sole discretion of the researcher or even the small community of researchers that initiated this process. Although, if we say this, we must also acknowledge that it is not just a semantic or discursive utterance that is circulating; it is a real thing because "[l]aboratory scientists make autonomous facts."[42] The better made the artefact is, the more autonomous it becomes. Grove put is this way: "[k]nowledge is an artefact created by us; but once created it exists outside ourselves; it possesses a certain autonomy; it affects us (it has unintended consequences), and we can affect it."[43] We certainly add to reality and help create it (in this sense it is subjective—a poor word choice and one that we are going to do away with soon), but it also poses problems and surprises us. That is, it adds to us; it affects us; and it has unintended consequences (in this sense it is objective—another poor word choice that soon will be excised).

---

[41] Latour (1999), 156.
[42] Latour (1999), 281.
[43] Grove (1989), 22.

The ideas of partial reality and constructed autonomy form what Latour calls propositional configurations.[44] Artefacts gain existence and autonomy by associating with more and more elements, and, conversely, lose reality as they shed collaborators. These associations and dissociations form propositional configurations that propose themselves as artefacts in our common world. This gaining and losing of elements means that configurations can be larger or smaller depending on the amount of reality and complexity the artefact already has (by virtue of its being an identifiable proposal it must already have some reality). The more reality a propositional configuration has and needs, the more serious a candidate it is for entry into the new collective (that is, the new common world). As individuals who engage propositional configurations, speaking subjects are yet another element in the artefact's spatiotemporal envelope. In a sense, subjects themselves are produced by the various artefacts they engage with. For example, we can readily understand someone as an ecologist when she acts in a manner consistent with the dictates of ecology. The same is true for outdoor recreation scholars and users.[45]

As an example of the associations and dissociations in propositional configurations, I look at Wall and Wallis' discussion of camping in Canada. They noted that as camping grew in popularity, along with the number of automobiles, problems began to arise. Many of these early campers camped along the roadside—there were no facilities and many enjoyed the feeling of roughing it beside a stream. But by 1921, Salamon observed that

---

[44] Although Latour calls these chains, I will use the term 'configuration' because it is a more robust metaphor that allows greater complexity among the elements comprising it.

[45] It should be clear, when we stop to think about it, that if all artefacts are configurations, to say that a speaking subject forms one link in another's configuration is to ignore the fact that that subject is itself also an artefactual configuration. It is really only some of the links of the subject-configuration that intersects with the ecology- or OR-configuration. For simplicity's sake, however, I have considered only one configuration at a time.

> various communities found [the camper] to be getting to be a serious menace. He was not always clean, and sometimes he left the most unhealthy trail of rubbish behind him. He polluted fresh streams, and many serious forest fires can be laid to his carelessness. It soon became clear that he must be segregated.[46]

In the early 1920s, the problem of campers was noted and more facilities began to be constructed to deal with this problem. However, the demand continued to outstrip the supply. In 1982, Wall and Wallis noted that by "the mid-1960s campgrounds became overcrowded and the overwhelming numbers of campers caused environmental deterioration. Even wilderness users began to feel the pressures."[47] Here is an example of how a propositional configuration changes when elements are added and subtracted from it. In 1921 polluting streams and leaving 'a most unhealthy trail of rubbish behind' was not considered environmental deterioration. Campers were a menace, not so much to nature as to the people who lived near the impromptu roadside campsites. The solution the locals supported reflects this claim. The locals wanted to remove the campers to another location instead of altering their behaviour to lessen the impact.

In 1921, the configuration looked different than by 1982 when Wall and Wallis wrote their chapter. Campers were a serious menace in 1921, and this representation contrasts strongly with the way campers were positioned in 1982. By 1982, campers were not menacing as much as ecologically unsustainable. Their actions while in nature had become central instead of the concerns of people living near the campsites. The Wall-Wallis configuration contained other links that were not there in 1921. For example, the developments that occurred in the 1960s, such as the publication of Rachel Carson's *Silent Spring*, must be included in the Wall-Wallis configuration. Earth Day, James

---

[46] M. A. Salamon (1921) quoted in Wall and Wallis (1982), 343.
[47] Wall and Wallis (1982), 345-356.

Lovelock's Gaia hypothesis, environmental lawsuits, films made about the environmental crisis, and scientists arguing about the negative impacts humans were having are all elements that did not exist in 1921. This is how the spatiotemporal envelope moves through time: as we passed from 1921 to 1982, the envelope changed, grew, and shrunk.

As a consequence, asking whether things were real before they were given meaning in discourse is to ask the wrong question. Even the idea of speaking about a pre-existing, non-discursive reality confuses more than it clarifies. To think that we can represent extradiscursive reality through language is an oxymoron. Instead, we should say that discourse has the power "to produce effects that, in time, are taken to be independent causes."[48] Furthermore, we must recognize that all artefacts are meaningful and hence they "may be polysemic (meaning several things at once), or they may be ambiguous (intimating several different meanings without specifying any one clearly), but they are never entirely nonsensical (radically devoid of significance)."[49] If so, then the common sense question, 'Is it real or constructed?', actually makes opaque the relations between humans and nonhumans because by pitting the two options against each other, we must posit this oxymoronic extradiscursive reality. How can something be real if by that we mean pre-interpretive or radically without significance? How can we refer to something that by definition has not been signified yet?

But if one refuses to ask the question, 'Is it real or constructed?', where does that leave us? Without claims of the really real, the fear is that we drift into a vast sea of relativistic nothingness where all claims are equally valid and "[t]he only way … to escape relativism [so the argument goes] is to *withdraw* from history and locality every fact that has been proven right, and to *stock* them safely in a nonhistorical nature where they have always been and can no longer be reached by any sort of revision."[50] The problem with this approach becomes apparent in the scare quotes, shifty/ing discourse, and paradoxes that arose as ecology and OR searched for that secret place where nature abides in its unsullied purity. There is, fortunately, another option.

---

[48] Kaufman-Osborn (1997), 122.
[49] Kaufman-Osborn (1997), 78.

Let us take as a starting point the idea that reality and construction are synonyms not antonyms. That is, the better made artefacts can be, the more independently they function. The tricky question now becomes how to distinguish between different classes of artefacts, specifically the human and nonhuman (nature in particular) artefacts without replicating Cartesian subject-object dualisms. The term *artefact* helps here because it occupies a theoretical space between the harsh Cartesian landscape on the one hand and the nebulous, utterly contingent one on the other. To begin, Kaufman-Osborn distinguishes artisanal artefacts (what we have mistakenly called 'objects out there') from other types: "[a]rtisanal artifacts are not first and foremost things we struggle to make sense of, but rather resources for making sense."[51] That is, when it comes to building the new collective (one that will replace the old modernist one), 'objects out there' (for example, animals, rivers, outhouses, tents, and so on) do not present themselves to us as ready-made things to wrestle with and make sense of. Instead, they are part of the contested process of making sense.

This is what Latour meant when he noted that to think nature is mute or self-evident and that ecologists and OR scholars simply speak for it is naïve. The more scientists speak, the louder the debate becomes, the more players get involved, and the less clear the outcomes become.[52] There will be no time in the future when our scientific knowledge becomes so vast as to return us to a simpler state. Instead of settling matters of fact once and for all, the discoveries of scientists about ecological crises have, if anything, fanned the flames of public concern, debate, and struggle. As part of the process for making sense, 'natural' 'objects' conspire with humans to speak a common world into existence. As poststructural theory points out, no one person controls language; nobody masters it. We are, actually, mastered by it. In turn, what we have seen as silent partners in the enunciation of the common world actually contribute much more to this process of making sense. We should say that scientists

---

[50] Latour (1999), 157.
[51] Kaufman-Osborn (1997), 48.

> have invented *speech prostheses that allow nonhumans to participate in the discussions of humans, when humans become perplexed about the participation of new entities in collective life*.... [W]hat is at stake here is only a simple translation, thanks to which *things become, in the laboratory, by means of instruments, relevant to what we say about them.*[53]

It is important not to overstate this proposition: neither Latour nor I am suggesting that "things speak 'on their own,' since no beings, not even humans, speak on their own, but always *through something or someone else.*"[54] This gives rise to speech assemblages instead of isolated speaking subjects. Nothing speaks completely on its own.

This process of real-*ising* the world may still be objected to on the grounds that in every step of the process all the actors are humans: scientists work in the labs, write the papers, and give the presentations; politicians give speeches and pass laws; activists hold protests and sign petitions; and managers develop and implement policies. However, we can go further in re-distributing speech among the actors. The nonhumans that participate with us in enunciating the collective world "first appear as matters of concern, as new entities that provoke perplexity and thus speech in those who gather around them, discuss them, and argue over them."[55] These artefacts reveal themselves slowly, in different (often conflicting) ways in response to the trials to which scientists subject them. What can be said about them, what is said about them, is a property assigned not only to the human actors but also to the artefacts. True, they are not equal partners, but they are equally necessary to the speech process. When a new artefact suggests itself to the emerging collective world, we should not ask whether it is natural or cultural. In place of this, we could ask "whether the propositions that compose it *are more or less well articulated*."[56] The above example of environmental deterioration and camping is a good illustration of this process. Over time, this artefact became more and more articulated with more and more collaborators until it was admitted into the collective. It is the ability to speak more fully, more complexly, and more clearly that counts in this collective world.

---

[52] This can be seen in the debate surrounding the climax model. After Clements developed his comprehensive 1916 model, the debate actually increased. Nature showed itself in various forms, shapes, sequences, and locations that all made serious claims for admittance into our collective. See Chapter Two for examples of this debate.

[53] Latour (2004), 67.

[54] Latour (2004), 68.

This process of speaking does away with the old questions of whether an artefact is natural or cultural, and would have significant implications for OR's claim that wilderness is authentic nature. We do not judge something's value or place in the collective based on its degree of naturalness or culturalness. Latour outlines the process that a proposition to the collective undergoes before admittance. Every new proposition, he argues, first

> induces perplexity in those who are gathered to discuss it and who set up the trials that allow them to ensure the seriousness of its candidacy for

---

[55] Latour (2004), 66.
[56] Latour (2004), 233.

> existence; it demands to be taken into account by all those whose habits it is going to modify and who must therefore sit on its jury; if it is successful in the first two stages, it will be able to insert itself in the states of the world only provided it finds a place in a hierarchy that precedes it; finally, if it earns its legitimate right to existence, it will become an institution, that is, an essence, and will become part of the indisputable nature of the good common world.[57]

These are the first four steps in this process of convoking the collective world. First, a proposition perplexes (surprises) all those involved with it. Second, if it can make its various actants speak forcefully enough, this proposition can demand to be taken into account by all those involved with it. Thus, its spatiotemporal envelope expands. Third, as its envelope expands, it must find a place within the hierarchy of the collective. Finally, as it finds its place in the existing structure and gains solid footing, we can say that its envelope has expanded enough for it to be considered as a real, bona fide member with a legitimate claim to exist within the collective world. These four steps form one half of the evolution of the collective.[58] They show how new propositions get internalized and become members.[59] I want to draw attention to the way both the process of accepting and internalizing can be tied to Foucault's rules of discourse.

---

[57] Latour (2004), 123.

[58] It should be noted that although Latour's process has much to recommend it, I have, nevertheless, some misgivings about aspects of it. For example, step two requires the configuration to fit itself into the pre-existing hierarchy of the collective. While this does seem to work, it could hardly be the only format that the collective contains. Some elements in our collective are arranged according to non-hierarchical discursive practices. In the final step, Latour notes that the entity must gain the status of an institution in order to be a full member of the collective. Again, it is no doubt the case that some entities appear to have a fixed essence, which is an achievement that makes it almost inevitable that the entity will become a full member of the collective. However, this does not at all seem exhaustive of the possible ways a configuration can become an institution in the collective. Furthermore, even with the most entrenched members of the collective, there is always the possibility for dispute. Although Latour calls institutions part of the indisputable nature of the collective, they are never completely immune from dispute.

[59] There is another process that forms the twin of this process of internalization. Externalization explains the process that propositions undergo when they are ejected from the collective or when their membership is refused. Although externalization is closely connected to internalization, the focus of my work is on how propositions come to exist in our collective and, therefore, the process of externalization lies, in the main, outside the scope of the present work. It should also be noted that both these processes are not either/or situations. Propositions can be part of the collective to varying degrees; they can also be ejected to varying degrees. Something does not simply exist: it has shades of existence.

Artefactual configurations are not accepted or rejected randomly; there is a pattern to the process and identifiable forces at work. There are some positions that humans can occupy, for example, that afford them more ability to engage certain artefacts in particular kinds of powerful conversations. For instance, ecologists speak more scientifically and, consequently, more authoritatively about nature and natural processes than the lay person can. This is not to say that ecologists get at the truth more clearly; it is to illustrate that they are in a position of greater ability to initiate and further the process of internalization of certain types of propositions related to the environment. Propositions that can be shown to be in sustained conversation with a number of experts often have an easier time entering and remaining in the collective. Of course there are a number of other forces that must be considered. The case of the Kyoto Accord serves an example where ecologists and scientists have clearly identified global warming as a serious applicant for this collective. However, not every nation has accepted this. In Canada, as of early 2008, the accord currently exists as an artefact whose institutional solidity is tenuous. It may be accepted in certain circles, but official government policy has not admitted it fully.

The patterns visible in the internalization of propositions in our collective reflect what Foucault has called the rules of discourse. He suggested three main types of governing rules: external or rules of exclusion (for example, division and rejection, prohibition, and the opposition between the true and the false), internal rules (that is, "rules concerned with the principles of classification, ordering and distribution."[60]), and the rules governing speaking subjects (that is, "those limiting [discourse's] powers, those controlling its chance appearances and those which select from among speaking subjects."[61]). These three types of rules operate in both ecology and OR. Internal rules are an important aspect in understanding how certain propositions come to be accepted into the collective. There are three types of internal rules: commentary, author-function, and the disciplines.

---

[60] Foucault (1972b), 220.

Internal rules are concerned with the classification, ordering, and distribution of knowledge in a discourse. Foucault first distinguished between the commentary and author-function. The former orders statements and knowledge based on reiteration and repetition, while the latter serves as an organizing principle or seat of coherence for a group of works. Whereas the author-function gives discourse a sense of cohesion by identifying a body of work to an individual, commentaries achieve cohesion through repetition and sameness. In ecology and OR, the internal rule of commentary encouraged studies that simultaneously repeated what had always been said while at the same time saying something new. Throughout the twentieth century, ecology, for instance, emphasized linear and progressive models because these could be used more easily and productively in prediction and control. Prediction and control, I have already suggested, were part of the surface from which ecology emerged and crossed the threshold of scientificity. In this sense, prediction and control are one manifestation of commentary in ecology. That is, the theme of prediction and control was repeated over and over, but with new objects of attention and with new conclusions. As endlessly repeated commentaries, prediction and control contributed to the formation of our collective by influencing the admittance criteria for new propositions: those that aligned themselves with these tropes were the ones that appeared more readily and forcefully in the discourse.

---

[61] Foucault (1972b), 225.

The disciplines constitute another type of internal rules governing discourse. For Foucault, "[d]isciplines constitute a system of control in the production of discourse, fixing its limits through the action of an identity taking the form of a permanent reactivation of the rules."[62] Scientists were constrained and enabled by their discipline as it continually reactivated its own internal organizational procedures and processes. In outdoor recreation, knowledge was produced and arranged according to its own disciplinary pressures that emphasized an understanding of natural nature as pristine, authentic, usually vast, and wild.

In addition to internal rules, rules of exclusion help to explain the pattern to the incorporation of propositional configurations in ecology and OR. Both ecology and OR operate under the premise that nature is separate from humans and therefore valuable. This separation forms a foundational division and rejection in these disciplines; that is, humans and nature are divided and then the human side is rejected as inherently damaging. Closely connected to this is the practice of conceptualizing natural nature as balanced when humans did not interfere. The balance/disturbed division presents another manifestation of the internal rules of ecology and OR. Both ecologists and OR researchers conducted experiments according to this division and rejection. This rule of division and rejection influenced the classification and arrangement of various kinds of nature in ecology and OR (in OR, along the Recreation Opportunity Spectrum, for example).

Internal rules, external rules, and the rules governing the speaking subject influence the kinds and amounts of propositional configurations proposed by ecology and OR. Ecological experiments were designed in accordance with the governing rules.

---

[62] Foucault (1972b), 224.

Studies of animal metabolism, watershed energetics, population dynamics, and succession and climax models followed this division and rejection coupled with the commentary on prediction and control. Practices and policies that adhered to these rules were also established in OR. The LNT strictures, for instance, reflect such a division and commentary. Backcountry design betrays its adherence to these rules, too: trails are built to bring people into contact with natural nature.[63] Particular trail designs, LNT standards, watersheds, forest and stream ecosystems, and normative backcountry behavioural regulations are all examples of propositional configurations in ecology and OR that can be understood through Foucault's rules of discourse. These rules make certain types of propositions much more likely and powerful. They also link propositions together. For example, underlying all the above propositional configurations is the same division and rejection, the same use of commentary, and similar rules privileging certain speaking subjects.

Propositional configurations that broke these rules of discourse were discriminated against before they could even give voice to their application for admission into our collective. In this sense silence becomes their sign. In ecology and OR the ghosts of these propositions haunt the discourse. In OR, for example, nature has been seen mainly as a balanced system. Thus, discussions of chaotic ecological systems are absent because they have already been discursively divided from notions of stability or balance and then rejected. In part this absence is connected to the internal organizational structure of OR discourse: the discursive regularities that tended strongly toward conceptualizations of nature as balanced formed conditions of thought and were therefore activated before any one individual scholar conducted any experiments.

---

[63] For example, see Cowan (1970) who suggested that salt licks be approached from "the appropriate side" and that "the pondweed bed where moose will be seen in the morning and evening [will be] exposed just enough and from the best direction for viewing and photography," 324.

The absence of chaos models is also a result of the various rituals used in OR to study nature. For example, the use of quadrats to measure hiking impacts set a base line from which to compare the post-trampling changes. This base line functioned as the normative state of the system and efforts that succeeded in returning it to that state helped to create it as a static system. The design of the experiment contained rituals that made the production of the statements, knowledge, and concepts of chaos theory that much harder to incorporate. The rules of discourse often operate in conjunction with each other. Consequently, in addition to the rituals that discriminated against chaos theories, there existed a prohibition against humans-as-natural because the quadrat created a space inside which human impacts were considered fundamentally different from other similar types of impacts (for example, animals walking through the quadrat). In fact, in trampling studies the ritual of walking repeatedly across the quadrat constituted behaviour that reinforced the separation of humans from nature by making visible the negative impacts of people. This, in turn, supported the idea that the further off the base line the system moved, the more damaged it became. So while the rules of discourse are always productive in that they enable studies such as these to be conducted, they also limit studies to those that complied with the discursive conditions.

Through these governing rules, propositions get accepted or rejected to varying degrees in varying circles within our collective world. The rules of discourse are not the only element involved, of course. Nonhumans have their own influences on the process of internalization. We can come to understand these influences through discursive rules and practices, but this access always remains incomplete because once we claim to understand the role nonhumans play, we have already entered into discourse. Rendering nonhumans in terms of discursive practices is to silence them to a degree. This represents an absolute limit. The only way to even partially access the role nonhumans have in convoking the collective is through traces and hauntings. Nonhumans act, and while these actions have effects, nonhuman actants never fully appear in discourse. For example, the surprise scientists often encountered is a result of what nonhumans do. The process of making sense out of that surprise represents the incorporation of nonhumans into the discursive structure of science. Remaining surprised might in some way be truer to the nonhuman actants; however, surprise, by its very nature is somewhat incomprehensible and serves as a reminder of the incompleteness of our access.

What is of particular note here is the composition of propositional configurations. The question is not whether an artefact is human or nonhuman. The question is about the amount of reality it has as a propositional configuration. It will either lose or gain reality as the cycle of internalization continues. If we begin to see each propositional configuration as a unit (object/concept/strategy), we can do away with the troubling divisions of real from non-real, things from meanings, and material from discursive reality. Every propositional configuration is subject to the rules of discourse *and* they all contain what we used to think of as simple, real, material objects and discursive meanings. Hence, meaning and existence are coupled, not at a certain point in the real-*isation* of artefacts, but through and through the process. That is, as artefacts are admitted into the collective by the rules that govern the disciplines, they gain both meaning and existence. There is no point at which we can say, 'there, now it is really real, really here' without having to invoke the power of interpretation and meaning. On the other hand, there is no point at which we can say, 'there, now it has disappeared into a world of pure meaning with no reality.' As long as there is meaning, there is existence to one degree or another. As long as something exists, it has meaning to some degree or another.

The silences that are revealed in the discourses of ecology and outdoor recreation are quite suggestive. As mentioned above, nature was most often thought of as balanced, which created a silence that can be read as the trace of chaos and indeterminacy. Ecological theory, however, has recently undergone significant changes. Beginning in the early 1970s, new paradigms in ecology appeared. These paradigms illustrate a dramatic shift away from ideas of the fixity of knowledge to an orientation much more open and accepting of paradoxes. They also represent a new set of discursive practices vis-à-vis the rules of rituals, doctrines, and divisions/rejections. To better understand these changes and their implications for outdoor recreation, in the next chapter I outline the development of chaos modelling, patch-dynamics, hierarchy theory, and landscape ecology. These developments in ecology offer alternatives to the extant governing rules in OR discourse that privilege notions of static, balanced ecological systems without evidence of human interference. Understanding the nature of paradoxes, the process of internalizing propositional configurations, the insights and challenges of poststructural theory, and the newer ecological models enables us to develop a deliberately paradoxical ecology and OR and to see what benefits of this are. This is the task of Chapter Seven where I offer some thoughts on how ecology and OR might produce and organize knowledge were they to re-evaluate their stance on paradoxes by engaging different rules of discourse for themselves.

CHAPTER 7: PARADOXICAL ECOLOGY AND OUTDOOR RECREATION

Kicking stones is all very well,
but we can learn more when nature kicks back ~ J. W. Grove[1]

There are four sections in this chapter that build toward the construction of a deliberatively paradoxical ecology and OR that incorporates some of the implications of poststructural theory, recent theoretical developments in ecology, paradoxes, and science studies. My contention here is that an uncritical use of the scientific method, coupled with a sceptical or even dismissive stance toward poststructural theory and paradoxes, results in ecology and OR being less scientifically rigorous than they claim to be. Ironically, in ecology and OR, the unquestioning reliance on science has actually resulted in each being less scientific.

The ability science has to make positive changes in the world is plain. This potential for positive change can be applied to the scientific method itself in an effort to make significant and needed alterations in various scientific disciplines—in this instance, ecology and outdoor recreation. Moreover, it is a methodology that ecologists and OR scholars are already familiar with and, thus, does not require a significant paradigm switch.[2] The approach I advocate acknowledges (1) that nonhumans are involved in crafting our world, (2) that paradoxes are elements that, when examined, help us see the limits of our knowledge, and (3) that science, far from being only a normative and restrictive process, can open the discussion to non-scientific or non-rational elements. In fact, the science of ecology itself began to investigate the importance of the unpredictable and non-rational in the 1970s. Studies of chaos theory and patch-dynamics, to name two such developments, challenged the idea that nature behaved predictably or even rationally.

---

[1] Grove (1989), 8.

[2] Different cultural perspectives could also be used to analyze of the limits of ecology and OR. In fact, local ecological knowledge (LEK) has already been used as a means to accomplish this. Many studies have shown the similarities and differences between scientific ecology and local ecological knowledge, as well as how each perspective can challenge and inform the other. LEK should be seen as a complementary analysis to the one I present here: neither is exhaustive of the subject and in some ways one requires the suspension of the other. For an example of a comparison between LEK and ecology, see Gilchrist and Mallory (2007). I have chosen to remain within the scientific paradigm in part because ecologists and OR researchers are already familiar with it.

In order to better understand the role the unpredictable and non-rational can play in science, I outline some of the newer theories and models in ecology that do not fit the older, more normative models of a static or balanced nature. Developments in ecological knowledge that are particularly pertinent here include stochastic modelling, non-linear systems theory, chaos theory, patch dynamics, and complexity theory. These models all present a similar challenge to ecological methodology: they suggest that reality is actually much more complex than earlier understandings in ecology would have it. To see this shift, I begin with a discussion of the limits of the older, more normative form of ecology and outdoor recreation. The development of newer models and theories in ecology is, in part, a response to the limits it encountered more frequently as the twentieth century wore on. Outdoor recreation, because it shares much of the same discursive structure and rules as ecology, encountered similar limits; however, the developments in ecology have not yet made their way into outdoor recreation scholarship in any systematic fashion.

The possibility of incorporating newer ecological theory into OR is one main benefit that arises from a deeper understanding of newer ecological theory; however, because of the complexity of the models, concepts, and theories involved, the benefits contemporary ecological theory offer for OR are not straightforward or always clear. In many ways, the benefits are a process rather than a discrete product. In other words, an understanding of newer ecological theory results in a different orientation to knowledge production and the study of human-nature interactions, rather than in any specific change in policy or practice. A change in orientation may very well lead to changes in policy and/or practice, but it is impossible to say what those changes will be until our orientation has changed. We need to understand the implications newer ecological modelling has for how OR approaches the study of nature before we can begin to see what those changes mean in terms of policies and practices.

While recent developments in ecology hold the potential for changing OR, they are not immune from critique themselves. Consequently, the third section of this chapter initiates a poststructural and science studies critique of recent ecological theory. A conversation between poststructuralism, science studies, and ecology provides a productive space in which OR can engage in a dialogue about actants and propositional configurations. While it is the case that ecologists have grappled with tricky questions of chaos, chance, and randomness, and that OR could stand to learn from their attempts, the potential also exists for both fields to become more sophisticated in their understanding of the role nonhuman actants have in the process of building our collective. Propositions are admitted into the collective through a complex process that includes nonhumans, discursive regulations and regularities, and human beings. The separation of culture from nature that both ecology and OR presume clouds their ability to see how humans are implicated in the production of an independent reality that is then perceived as natural nature. I devote the third section of this chapter, then, to examining the interplay between elements in propositional configurations that have traditionally been distinct from each other. That is, I examine how nonhuman actants can influence the production of knowledge in ways that even newer ecological theory does not account for. By challenging ecology to account more fully for nature's agency and paradoxes, I hope to free scientific discourse from unnecessarily restrictive ideas about the nature of reality, as well as re-work what is currently seen in ecological and outdoor recreation literature as an indisputable split between nature and culture.

The final section in this chapter offers some suggestions for what a deliberately paradoxical ecology might look like and the benefits it proffers for outdoor recreation. In short, what remains, in this last section, is to pull together the various critiques of normative science raised by science studies, the challenges of and to poststructural theory, and more recent ecological knowledge to suggest an alternative epistemo-ontological[3] structure that OR might engage. To a large measure, the challenges raised by poststructuralism, paradoxes, and science studies indicate that concepts of agency and object, actor and acted upon, and cause and effect are not actually opposites. Consequently, we cannot isolate the discursive part of nature that is produced solely by our ideas about what it should look like. At the same time, however, we cannot show that particular, objective element in nature that helped produce what we now call 'wilderness.' When we think that it is possible to isolate such variables, we can only ask questions such as 'What are the objects of nature?' or 'Who are the actors involved?' We can never ask questions such as 'What are the actors?' or 'Who are the objects?'

This limitation on the type of questions ecology and OR can ask has made it difficult for them to provide satisfying answers. On the one hand, they both have been wrestling unsuccessfully with answering 'What are the objects of nature?' On the other hand, 'Who are the objects of nature?' is a question that makes little sense within the normative discourse of science and OR because it conflates actors and objects in ways intolerable to modern society's power-knowledge regime. This conflation is precisely the problem. The modern power structure requires the separation of agents from objects, except that we can no more separate subjectivity and objectivity as wholes unto themselves any more than we can remove elements of either from a propositional configuration. I suggest a deliberate and direct engagement with a paradoxical position that recognizes the necessity and impossibility of working with propositional configurations.

---

[3] This term indicates that the alternative I speak of does not make the same assumptions around the nature of reality and the status of our knowledge of that reality that modernity currently makes. Therefore, it is inappropriate to speak in terms of ontology versus epistemology.

The Limits of Ecology and Outdoor Recreation

How wonderful that we have met with a paradox.
Now we have some hope of making progress. ~ *Niels Bohr*

By the 1970s, ecologists began to criticize many of the mainstays in ecological theory.[4] The ideas of equilibrium, stability, balance, and harmony, integral components of most ecological thinking since the mid-1800s, were coming under question. As scholars and researchers reflected over the previous century, they began to see the limitations of earlier models and theories. In general, these limits reflected an unsubstantiated reliance on ideas of balance and stability. Natural systems were almost exclusively classified and studied as systems that, when healthy, were either working toward or had achieved stability.

---

[4] In 1974, for example, May found that the more diverse a system was in terms of species richness (biodiversity) the *less* stable it was. This overturned a theory dating back to the 1920s and the founder of animal ecology, Charles Elton. May was challenging a nearly universally accepted principle in ecology. Conservationists, too, had adopted the connection between diversity and increased stability wholeheartedly. As did OR, which seems to have refused to give it up, even today. According to Worster, May claimed that "wildlife populations often did not follow some simple pattern of increase, saturation, competition, struggle, and balance. One could find, to be sure, many stable points and cycles, but one could also find everywhere the hand of chaos" Worster (1994), 409.

Mature forests and ponds, for instance, were those that lasted relatively unchanged throughout the years. Systems that exhibited change, on the other hand, were still developing toward their mature state: just as children develop into adults, immature ecological systems develop into mature and stable ones.

Those ecologists that critiqued earlier models often drew a distinction between

> determinism and indeterminism, stability and instability, stasis and change. These dichotomies, so it is claimed, reflect antithetical intellectual positions deeply rooted in different cultural matrices. Historically, these ecologists contend, their discipline is grounded in a dogmatic commitment to the idea that nature is in equilibrium; only recently have ecologists recognized that the living world is characterized by pervasive disturbance and instability.[5]

As a result of such distinctions, ecologists began to focus on models and explanations in which disturbance was a central component.

The traditional conceptualization of balance has been criticised by some ecologists lately. In 1992, McCoy and Shrader-Frechette suggested that

> [a]mong traditional natural historians, nature was in balance when no changes could be detected in the identities or population sizes of the component species of a biotic community. The frame of reference for discerning lack of change was the period during which the community was observed, a period which usually encompassed a few years to decades. Interruptions of the balance resulted from 'disturbances' (e.g., fire, timbering, cultivation) that promoted detectable changes in the environment, giving rise to the common wisdom that nature would be in balance except for the meddlings of humans.[6]

Even by the time ecology had crossed the threshold of positivity early in the twentieth century, the concept of a balanced nature was a well-rooted scientific concept. It remained a central theme in the natural sciences throughout the 1900s, even as more and more evidence accumulated against it. Finally, by the 1970s, ecologists began to question certain elements of the model.[7] As a result, many began to speak of dynamic balance (and later of dynamic stability) instead of static balance.

---

[5] Hagen (1992), 3. See also DeAngelis and Waterhouse (1987).

[6] McCoy and Shrader-Frechette (1992), 184. Notice the scare quotes here.

Inspiration for these modifications was often drawn from other disciplines, especially physics and mathematics, “in part because laws and theory are not well established in ecology.”[8] As the 1900s worn on, ecology developed more complex and varied explanations for stability. Researchers discovered instances of nature behaving in ways extant models of balance and stability could not explain. By 1975, attempts to account for these behaviours had resulted in the accumulation of at least seven different conceptualizations of stability: (1) constancy: the lack of change in some parameter(s); (2) persistence: the survival of a system or component of it; (3) inertia: the ability of a system to resist external perturbations; (4) elasticity: the speed with which a system returns to its former state; (5) amplitude: the area over which a system is stable; (6) cyclic stability: the ability of a system to oscillate around some central point; and (7) trajectory stability: the ability of systems to move toward a final end state despite starting in different states. These ideas about stability/instability are not compatible with each other, nor do all ecologists agree on what measures are significant. Orians, who suggested these seven types, added that “ecological advice on matters relating to community stability is highly intuitive,” despite that fact that “scientific ecology should be able to provide better advice to decision makers who are responsible for … preservation.”[9]

Beginning in the 1970s, ecologists began to comment more and more on the limits to the concept of a balanced nature. In OR, on the other hand, the notion of a balanced nature continued to be endorsed.[10] Most commonly in OR, wilderness has been conceptualized as a system in balance, except when humans interfere. The minimum impact standards, for example, repeatedly stress the damaging effects wilderness users have on the ability of a system to persist or recover. Concerns over loving nature to death reflect this fear: OR scholars are worried that humanity's continued use will eventually destroy the balance of the very thing they love. If, however, ecologists are correct and stability/balance is a far more complex and muddy concept, then OR's conceptualization of a healthy ecological system is mistaken. The implication that arises from the recognition of the limits of the traditional conceptualization of nature as balanced is not that nature is unbalanced and that all changes are acceptable. The suggestion that nature may not be balanced does not authorize a hands-off position that allows any and all actions in wilderness. Instead, the implication is more subtle. I contend that what is at stake here is not just the preservation of wilderness, but also the preservation of particular relationships between various stakeholders (including nonhumans). As we will see, adopting more current scientific views actually strengthens OR's argument that wilderness areas are unique and special landscapes that deserve to be protected. Sadly, for the most part, OR has consistently favoured the concept of a balanced nature.

---

[7] The details of the continued use of ecological concepts (for example, community models, successional patterns, and understandings of associations, formations, or population) even in the face of contradictory evidence were presented in Chapter Four.
[8] McCoy and Shrader-Frechette (1992), 186.
[9] Quoted in McCoy and Shrader-Frechette (1992), 187.

Recently, ecology has become aware of another limitation to traditional ecological theory, this time regarding the species richness-stability hypothesis. As with the concept of a balanced nature, OR has not made the same realisation. In the 1960s and 1970s, ecologists began to test the idea that an increase in species richness increases ecosystem stability. The logic runs thus: systems with more component parts are more able to absorb stresses than systems with only a few components. For instance, the mono-culture crops of trees now found in many of Canada's national parks (in part, a result of fire suppression policies) are far more susceptible to devastating insect epidemics. Where a greater variety of tree species exists, resistance is also greater because insects usually do not target all species equally. No doubt, a similar hypothesis regarding the species richness-stability hypothesis motivated OR researchers as they turned to ecological literature in their commendable efforts to preserve and protect wilderness areas. Minimum impact standards, research agendas, company policies, and management strategies all reflect the belief that wild nature is in balance and that this balance is best promoted, protected, and promulgated by increasing species richness.

---

[10] Lister and Kay (2000), for example, noted that "[i]t has long been assumed that there is an inherent 'balance' or stability in nature, which biodiversity helps to maintain. This, it has been suggested, is a strong argument for conserving biodiversity. Many policies have been based, at least in part, on this assumption (for example, protected areas and parks management plans). However, this notion of stability is difficult to defend in scientific terms. Even defining what is *meant* by 'stability' is difficult" (193). Notice the use of scare quotes again here. I do not wish to suggest that the advent of models addressing randomness solved all problems associated with notions of stability and balance. Ecologists still wrestled with these concepts, but they were at least overtly acknowledging the contradictions. Further support for the idea that public opinion and ecological knowledge differ regarding biodiversity can be found in Vanderlinden and Eyles (2000).

As ecologists began to test the link between species richness and stability, they found it to be tenuous at best. If wilderness ecosystems were to be saved from the damaging effects of users, ecologists began to realise, then increasing the species diversity of those systems was less helpful than originally thought. Nonetheless, the idea of the species richness-stability link continued to grow in popularity and power. This link "has been influential to the extent that it was cited as more or less of a *cause* in much of the literature discussing diversity and was repeated as more or less of a *fact* in textbooks, conservation pamphlets, and the printout of environmental institutes."[11] Unfortunately, popularity and power are not restricted to correct or accurate positions alone. As we now know, "the pioneering work [of these ecologists] and subsequent mathematical analyses failed to demonstrate any causal link between species diversity and community stability."[12] Nevertheless, the idea that species richness leads to stability remains an integral component in much of the literature on minimum impact today.

---

[11] Goodman (1975), 240. For example, see Pimentel (1966) who remarked that stability promoted resource conservation because "[c]ommunity stability increases as the community approaches the climax stage of succession," which often has a higher level of species richness (29). Wu and Loucks (1995) noted that "the balance of nature idea and the classical equilibrium paradigm have had profound influences on applied

As a result of the failure to find convincing support for the species richness-stability link, ecologists dropped it in favour of the complexity-stability link. However, this switch to complexity instead of richness did not resolve the problem. "Despite numerous attempts to link some measure of complexity with stability, the matter remains poorly resolved at present [1992]. One problem is understanding how stability at the population level relates to that at the community level…. A second problem is measuring stability."[13] McCoy and Shrader-Frechette acknowledge two further, under-discussed problems with this link between complexity and stability: (1) there is a large number of meanings that have been attributed to the stability concept and (2) the temporal and spatial scale over which stability is assessed changes the link between complexity and stability. Changing from species richness to complexity has not settled the matter of stability. In the end, the idea of stability remained highly problematic in ecology.

In terms of outdoor recreation, the idea of a stable nature enabled the development of many protected area policies, management goals, and minimum impact practices. The realisation that ecologists question whether or not nature is stable should not undermine the accomplishments OR has achieved using this concept. Minimum impact policies and

---

ecology, especially on nature conservation, as they have led to the supposition that 'nature knows best'" (442). This supposition is certainly a commonly expressed idea in outdoor recreation.

[12] McCoy and Shrader-Frechette (1992), 187.

[13] McCoy and Shrader-Frechette (1992), 188.

practices, for example, are very effective in reducing the negative consequences of overuse. The problem, then, is not with OR's goal of protecting wilderness as much as it is with the specific conceptualization OR produces/requires of wilderness. It turns out, for example, that many national parks in Canada are not considered ecologically healthy.[14] By encouraging minimum impact standards and management plans for parks and other protected areas that are based on the notion that natural nature is stable, OR has become part of the reason why these areas are suffering. For example, in the 1997 State of the Parks Report, Parks Canada reported on the percentage of historic fire cycle left for each park. Only the two parks with unreliable data scored one hundred percent. The next highest rating was less than fifty percent, which only two parks received. After that, the percentages range from less than thirty down to less than five percent. In other words, most of Canada's national parks contain (significantly) less than thirty percent of their original fire regime. This loss is largely the result of fire suppression policies that are based on the understanding that nature is stable and fires are a disturbance.[15]

Current ecological theory does not debate whether or not nature can ever be stable; it acknowledges that nature is sometimes stable. What is debated now is how prevalent instability is and how to conceptualize the non-stable aspects of nature. This debate, however, does not arise in OR. This absence is lamentable because recent ecological theory could provide OR with alternative models for understanding natural nature that could change both how minimum impact standards are developed, implemented, and disseminated, as well as how protected areas are managed.

---

[14] See Gordon Writing Group (1997). For example, in 1997 only one national park in Canada received the highest rating on ecological integrity; however, this park was established so recently that data were not reliable and, thus, it was discounted. Furthermore, out of the thirty-six other parks listed, four were rated at the lowest level of ecological integrity (five on a scale of one to five), eighteen were listed at the second lowest level (four out of five), ten at level three, and four at level two (one of which was also too recent to contain reliable data). In sum, over half of all parks with reliable data had, by 1997, received the first or second lowest score possible.

[15] Of course, the pressures from businesses, tour operators, and local residents to eliminate fires around residential and commercial developments were also influential in altering the historic fire regime.

McCoy and Shrader-Frechette summarized their history of the stability concept by quoting Connell and Sousa who concluded that

> [t]here is no clear demarcation between assemblages that may exist in an equilibrium state and those that do not. Only a few examples of what might be stable limit cycles were found. There was no evidence of multiple stable states in unexploited natural populations or communities. Previously published claims for their existence either have used inappropriate scales in time or space, or have compared populations or communities living in very different physical environments, or have simply misconstrued the evidence.... [R]ather than the physicist's classical ideas of stability, the concept of persistence within stochastically defined bounds is ... more applicable to real ecological systems.[16]

Connell and Sousa's comment supports my claim that ecology continued to rely on concepts and models even as more and more evidence accumulated against them. Over time, ecologists such as Connell and Sousa began to see the limits of traditional ecological discourse. In another example, Daniel Goodman noted, in 1975, that "the predisposition to expect greater stability of complex systems was probably a combined legacy of eighteenth century theories of political economics."[17] In OR, on the other hand, comments concerning the stability of nature remained commonplace. The evidence that was accumulating in ecology against this idea was not engaged with in outdoor recreation.

While some ecologists were critiquing their field along these lines, not every ecologist embraced the complexities of the debates. Eugene Odum, for example, was particularly influential in maintaining the traditional model of succession. As noted in Chapter Two, as late as 1969 he was still suggesting that ecological communities underwent clearly defined successional stages. However, cracks in the edifice had begun to appear. Paul Ehrilch and Jonathon Roughgarden, summarizing Drury and Nisbet's work (1973), concluded that, even in Odum's original studies in the 1950s, the data supported neither the claim for an orderly sequence of successional stages nor the suggestion that each stage prepares the way for subsequent ones as the classic Clementsian model had it.[18]

---

[16] Quoted in McCoy and Shrader-Frechette (1992), 188.
[17] Goodman (1975), 238.

I am not suggesting that Clements' model and Odum's support for it were completely misguided. Indeed, there appeared to be communities that followed this model quite closely. The appearance of such communities was, in fact, the problem: sometimes elements in the propositional configuration followed the models quite closely and at other times they did not. In 1987, Ehrlich and Roughgarden outlined the main changes in ecological modelling that occurred as it became clear that Clements' model was appropriate in some instances but not in many others. The turning point ecologists often put forth was Connell and Slatyer's development of an alternative model of succession in 1977. Connell and Slatyer suggested three pathways for succession: facilitation (essentially the classical model), tolerance (each subsequent species must be tolerant of the conditions formed by earlier ones), and inhibition (the antithesis of the classical model whereby each species inhibits the establishment of future ones).[19]

More recently, Benedetti-Cecchi sketched a history of succession and suggested that

---

[18] Ehrlich and Roughgarden (1987). According to Worster (1994), Drury and Nisbet "found no evidence of a progressive development over time: no trend toward biomass constancy, diversification of species, cohesiveness of plant and animal communities, or biotic control over the inorganic environment. Indeed, they found none of the criteria Eugene Odum has posited for mature ecosystems" (391).

[19] Ehrlich and Roughgarden (1987), 546.

> [h]istorically, ecological succession was described as an ordered, directional process (Clements 1928, 1936), but the idea of succession as a deterministic sequence of species replacement has been challenged…, and alternative models have been proposed to account for the variety of patterns identified in the field and in simulative studies…. Connell and Slatyer (1977), [*sic*] distinguished three alternative pathways of succession (the well known 'facilitation,' 'tolerance,' and 'inhibitions' alternatives) based on the effects that early colonists have on later ones. Later, Farrell (1991) proposed a model to predict the rate of succession…. This model integrated the three alternatives envisioned by Connell and Slatyer … with the results of experimental studies emphasizing the role of consumers in regulating ecological succession. More recently, Berlow (1997) addressed the question of how historical events affect succession, contrasting consistent patterns (canalized succession) with variable and unpredictable patterns (contingent succession).[20]

As we can see, successional stages proved to be far more complex than originally thought. As the theory changed, elements that were previously suppressed or absent came to the fore. Disturbance, in particular, underwent numerous shifts in location, function, and purpose relative to ecological modelling. Whatever position it came to occupy in these newer models, the notion of disturbance, while not always consistently used or defined, was becoming a central component in ecological modelling. More factors were seen as interacting in more ways, and more varied outcomes and fluctuations were being predicted from similar starting points.

For outdoor recreation, the 1970s marked the beginning of the development of minimum impact policies. As a relatively new sub-field in OR, it is not surprising that minimum impact research developed using more traditional ecological models and theories. However, this reliance in OR on models of stability continues to the present. Ecology has had over thirty years to build more sophisticated understandings of nonlinear development and alternatives to the stability hypothesis. By now, these alternatives are firmly entrenched in the discourse. In many ways, they are no longer new models. To say that OR discourse remained unaware of these developments because they were still new and contested is inaccurate. Instead, the discursive practices in OR made certain propositions more readily available while pushing others to the sidelines. The goal in OR is the protection of wilderness areas; thus, models that clearly and powerfully show the danger recreational users pose are far more helpful. The adopting a model of nature that suggested chaos, not the steady-state, was the norm would substantially complicates matter for OR. Because OR was in the early stages of developing minimum impact practices and guidelines, it made strategic use ecological theory that clearly supported the goal of minimum impact. I am arguing, however, that OR has reached a point where it can begin to question its own assumptions and look at contrary opinions and ideas. Ecological theory, from the 1970s onward, presents just this sort of opportunity for OR to grow.

---

[20] Benedetti-Cecchi (2000), 46.

Ecology in Outdoor Recreation

It is the theory which decides what we can observe. ~ Albert Einstein[21]

There are patterns visible in the discourse of outdoor recreation. These patterns are not the product of any one individual or even a small group of individuals collaborating on research projects. The examples I use below are intended to illustrate the way in which the discourse of OR funnels or channels thinking in particular directions. I am not positing that the blame for the superficial use of ecological concepts and models in OR discourse lies within the individual researcher as if he or she should have known better. Instead, I point to the way in which discursive forces and practices have influenced research in OR. Certainly, individuals can resist discursive pressures; however, resistance or change becomes that much harder when the contours of that which is being challenged remain unclear. That is, as long as discursive practices remain hidden, our options for change are limited. Thus, the first step in building the capacity for changing OR discourse is to raise these practices to the level of an object of thought. The examples that follow are intended in this spirit, as opposed to being a critique of individuals.

---

[21] Quoted in Malin (2001), 3.

In OR, the traditional view of succession and diversity still found expression ten years after Goodman had noted that diversity leading to stability was incorrectly cited as a fact in ecological literature.[22] In OR, in 1986, Curt Shirer argued that a "basic ecological principle is that diversity in an ecosystem equals strength."[23] Hammitt and Cole, in their influential book on the ecological impacts of wilderness recreation, also linked these ideas when they noted the following:

> [c]ommunities and ecosystems change with time. Succession is the relatively orderly change from young, simple ecosystems to more diverse and specialized older ecosystems. The more advanced stages of succession may be more resilient because their higher productivity, diversity, and higher degree of specialization promote more rapid recovery.[24]

These descriptions of succession, diversity, and development compare closely with the way ecologists used these concepts in the first half of the 1900s. By the 1970s, however, ecology had moved away from these understandings. Even by the end of the millennium, OR discourse was still employing definitions and explanations similar to those used in ecology eighty years earlier.[25]

Older ecological concepts and theories do not appear randomly in the discourse of outdoor recreation; they do so in a particular manner. Ecological discourse often contains sophisticated and detailed discussions of internal debates and conflicts. It would be inappropriate to suggest that OR should have a similar level of sophistication about ecological theory. Having said that, the discussion of ecological concepts or theories occurs in a simplified and problematic version in outdoor recreation discourse, and this could be contributing to the ecological problems wilderness areas in Canada are experiencing. At some level, this simplification is to be expected. On the other hand, though, when OR borrows models from ecology, some acknowledgement of the fact that there is little consensus in the parent discipline serves as a reminder that policies and practices based on the knowledge created by the model are only as valid as the model itself. An indication of the limits and complexities of ecological knowledge does not undermine the claims in OR concerning the importance of protecting wilderness areas in Canada. Instead, acknowledging the limits of ecological models could strengthen OR's position, because, as it stands now, OR is vulnerable to critiques that it does not fully understand the ecology behind the techniques, models, and theories it uses.

---

[22] Goodman (1975).
[23] Shirer (1985), 91.
[24] Hammitt and Cole (1987), 156.
[25] See Farrell, Hall, and White (2001) and Morton (2000).

This acknowledgement of the strengths and weaknesses of ecological models has not occurred in the outdoor recreation journals and monographs I examined between 1960 and 2007. Taken as a whole, OR discourse tends to simplify ecological knowledge into black and white (for example, there are specific behaviours and attitudes that are correct and others that are incorrect for Leave No Trace! graduates), and then uses that knowledge proscriptively. The problem lies in the disconnect between the actual state of the knowledge in ecology and the way OR uses that knowledge. It is strategic to simplify complex and ambiguous knowledge so that standards can be developed and enforced. Nevertheless, it is difficult to know, because these standards might be premised on inaccurate or incomplete ecological theory, how reliable or effective they are in attaining their goal without attending to the complexities in ecological discourse to some degree or other.

Of course, researchers in OR have a different objective than to understand the functioning of an entire ecological system. In ecology, the goal is often understanding for the purpose of predicting and then controlling ecological systems or elements therein. I am not suggesting that OR's objective is wrong. The goal of wilderness preservation and protection is laudable. In fact, my goal is to further OR's quest for increased wilderness protection. In the process, I discovered that OR actually operates in opposition to this quest in some instances. The examples I discussed in Chapter Four concerning the shifting of language from an ecological focus to a more human-centred one is one such instance. I suggested that OR's rationalization for enforcing minimum impact standards does not match the techniques commonly used to measure those impacts. In other words, OR discourse overtly links itself with questions of ecological integrity, but the details of the texts often appear to be more about the visual preferences of users. For example, a campsite with felled and scarred trees is not aesthetically pleasing to most backcountry users. This preference is not expressly about the health of the system, although it is quite possible that if enough trees are felled, the system would collapse. In other examples, OR users actually preferred situations that were ecologically damaging to wilderness. The preference of many users for denuded and hard-packed campsites over ones with more understory is an example of this. The research in OR notes the dichotomy between ecological health and user preferences; however, the discourse does not engage in a sustained discussion of the actual ecological consequences of such preferences.[26]

There are a handful of articles in OR that examine ecological issues in more depth.[27] For example, in 1997, Mattson discussed wilderness dependent wildlife and noted that "larger animals … tend to exhibit density-dependent responses in survival and reproduction only at densities near carrying capacity, and so have a limited ability to compensate for increases in mortality when they are already exploited."[28] A couple of comments can be made here. First, this is one of the more explicit and sustained engagements with ecological knowledge in OR. Even here, though, there is little discussion of the debates surrounding concepts such as population dynamics, which, at this point in time, had undergone serious revision in ecology. Mattson's use of carrying capacity as the marker for the upper limit of viability for larger animals suggests an understanding of population dynamics that is more in keeping with Elton's formulation, which dates back to the 1920s. Newer population models, as we will see, have moved significantly beyond Elton's work, but are not often included in OR. Second, this article does not engage with minimum impact issues to any great extent. It would seem, at first, that discussions of minimum impact would be compatible with ones on ecological integrity; however, the two rarely co-exist in one single work.[29]

Another example of OR's use of ecological knowledge is illustrated by Wall's 1989 chapter, 'Environmental Impacts.' In this chapter, Wall discussed the concept of succession (along with eutrophication and density independent population dynamics) in one sentence: "environments are not static but evolve naturally: vegetation succession takes place, lakes slit up and animal populations respond to changes in their habitats."[30] These are complex concepts in ecology with numerous variations, many of which are not compatible with each other. By not engaging with these debates, Wall leaves the reader with the impression that there is no conflict, no serious conflict in any case, as to the use and meaning of these terms. In a similar manner, models of homeostasis and stability are used in OR quite differently than in ecology. In the 1970s, ecology began to challenge the central assumption that nature was in homeostasis and, therefore, stable most of the time. OR literature, on the other hand, has continued to employ these concepts in ways reminiscent of ecological models from the early 1900s.[31]

---

[26] There are exceptions to this generalization, but they are by far the minority. Much of David Cole's research, for example, deals with ecological implications in more depth than others. Even Cole, though, uses ecological concepts and principles rather simplistically. See Christensen and Cole (2000), Cole (1995a, 1995b, 1997, and 2002), and Deluca et al. (1998) for examples.

[27] For example, Matthews and Knapp (1999)

[28] Mattson (1997), 35.

[29] This is a general pattern in OR literature. In the journals and books I sampled, none combined minimum impact with sustained discussions of ecological theory.

In OR comments such as "[t]he central message of modern ecology is that everything is in fact connected ultimately to everything else" occur, but do not capture the complexities, strategies, techniques, models and theories in ecology.[32] To state that everything is connected leaves off much, how things are connected and at what levels certain types of connections arise, for example. Not everything is connected in the same way, and some things are connected in more ways, more directly, or in ways that have multiple and often contradictory implications and effects. Outdoor recreation has yet to deal with these types of realisations that ecology has been grappling with for over thirty years now. In some cases, OR remains almost eighty years behind the developments in ecology.

---

[30] Wall (1989), 213. See Monz's (2002) study of the response of arctic plants to trampling for another example that illustrates the differences between ecology and OR in terms of understanding and evaluating the health of an ecological system.

[31] See Machlis, Field, and Campbell (1981). In this work, Machils, Field, and Campbell also presented succession almost identically with how it was first formulated by Clements in 1916: "[i]n general ecology, development of an ecosystem usually is described as a series of stages … whereby the community of plants and animals created conditions that lead to its own alteration" (203). Compare with Clements (1916) Chapter IV: Ecesic Causes, 63-78.

[32] McClaren (1990), 447.

As critiques of the related concepts of stability, complexity, and equilibrium arose in ecology, the realisation that these concepts need to be extensively re-worked dawned on scientists. One such reaction led ecologists to clarify the temporal and spatial scales at which the relevant processes take place. When ecologists view nature from this perspective, scale determines, in large measure, the answers to questions of stability and persistence. In other words, natural nature is neither stable nor unstable; ecological systems both persist and end. These realisations, in turn, led to the development of numerous models, theories, and perspectives in ecology. One common element in most of these is an attempt to deal with indeterminacy and randomness. Models that expressly addressed hierarchies of knowledge, patchiness, stochasticity, and non-linear developments became common in ecology, but not OR.

If, as ecologists suggest, nature changes dramatically and unexpectedly due to random events, then OR must re-conceptualize its models that are premised on predictable, biological, and evolutionary change. Unexpected and radical change now becomes much more common, which is not to say that all changes are good. Instead, it means that scientists must face the responsibility for ethics in their work. How they determine which changes are good and which are not is, in part, decided by the level they choose to observe from and the discursive practices that govern the production of scientific knowledge. As long as scientists saw predictable and linear change as a progressive, central component, as long as they saw nature as existing without a history, they never had to wrestle with this problem: they knew what change meant and what it should look like. Now this certainty has been lost. "If nature is not governed by unchanging laws, then how can we know what nature, and therefore ultimately we ourselves as part of nature, 'needs' or 'wants' over the long term?"[33] Attempts at understanding randomness, chaos, nonequilibrium systems theories, chance or stochastic events, hierarchy modelling, and patch-dynamics brought ecology into contact with the paradoxical nature of nature and our knowledge of it. While OR has not followed suit, these newer developments in ecology suggest a direction that OR could move in that would offer a more nuanced and complex understanding of wilderness and, consequently, how to protect it better.

New Ecology: Chaos, Chance, and Complexity

Everything lacks meaning ~ Friedrich Nietzsche[34]

Chaos and randomness are key concepts in current ecological theory. Ecologists began introducing these terms in the early 1980s. Eugene Odum, for example, listed random distribution as one possible option for population dispersal patterns. For Odum, random distributions followed "the 'normal' or bell-shaped curve on which standard statistical methods are based."[35] His remark contains a puzzling element: the use of scare quotes around the word *normal*. If random distributions were normal, ecologists should be able to use simple deterministic models to understand and predict the behaviour of natural systems. Furthermore, a key element in the definition of random is its lack of pattern. Surely a normal (without scare quotes) bell curve constitutes a pattern (a rather normal one at that) and, therefore, one would deduce, normal bell curves cannot be random. On the other hand, by using the appropriate techniques and tools, randomness can be ascertained as the governing characteristic of a distribution. It makes sense, then, to call this a random population distribution as distinct from other patterns with different governing characteristics. Under certain conditions, randomness can be seen as another pattern (one that is unrecognizable). Whether or not Odum intended it, his comment nicely illustrates the playing movement inherent in paradoxes. The idea of a random pattern seems necessary and accurate, as well as impossible and incoherent. Possibly due to a lack of alternatives, Odum deals with this tension by placing the word *normal* in scare quotes. These quotation marks do not offer a solution; in actuality, they do not even clearly define the problem. They are the problem without clearly exhibiting any details. It is a problem of paradoxes and cannot be clearly articulated.

---

[33] Ingerson (1994), 62.
[34] Nietzsche (1967 [1901]).
[35] Odum (1983), 344.

Concepts like stochasticity, randomness, chaos, and indeterminacy raise many challenges to knowledge in OR. Some of the challenges are methodological ones; however, some of them cross numerous boundaries such as philosophical, logical, scientific, mathematical, ontological, epistemological, temporal, and geographical.[36] Philosophically speaking, these concepts get to the heart of the matter of logic and reasoning. Defining something requires that it display a certain level of consistency in the attributes its definition is based on. When the only consistency is that there is no consistency in these attributes, we have a paradoxical situation. Thus, classifying a vegetation or population as a random pattern, as did Odum, because it displays no pattern begs a number of questions. A population is a definable unit; therefore, it must have enough consistent attributes to be recognizable as a unit. What does it mean if one of these attributes is a lack of consistency in its attributes? Is randomness another type of pattern, albeit a strange one? Can something random be predicted to any degree? In some ways our understanding of nature as chaotic is more comprehensive than previous theories based on stable and progressive views of nature. While this is an improvement, it is a strange one: our lack of certainty and closure is itself the sign of progress.

---

[36] Notice there are many boundaries, but there are also many types of boundaries here. This is part of the complexity of chaos and stochastic forces: they are multi-dimensional problems.

Before questions about randomness and chaos could be addressed, ecologists had to construct models that could distinguish between noise, error, chance, complexity, as well as stipulate the appropriate level of randomness. Without models such as these, there was simply no way to determine whether results reflected observer error, external noise in the system, randomness, or just complexity beyond current abilities to decipher. Thus, the paradox inherent in concepts of randomness and chaos lay largely dormant in ecology until such models were developed in the early 1970s. Until ecologists could be reasonably certain that natural systems were actually random, there was no need to deal with this paradox.

According to Worster, computers held out the promise of helping humans finally achieve a full understanding of nature. However, "the computer started to reveal a surprising degree of disorder, unperceived by pencil-and-paper calculators. Even the simplest equations could generate on the screen a motion that was so complex it appeared random."[37] Worster suggests that scientists now paid attention to what they had long managed not to see. I agree that the advent of computers was influential in the rise in chaos modelling. I do not, however, agree with Worster's explanation for the shift to an emphasis on chaos. Randomness, he said, was always there, but we turned our eyes away until such time as we were ready to see it and technology was powerful enough to show it much more clearly and forcefully. I would say, rather, that randomness is a unique proposition that arises because of the careful work scientists do to make it appear more and more autonomously. It is only today, because of this work done by previous scientists and the emergent entity that became known as chaos, that we can say chaos has been there all along. Until now, it never always existed.

---

[37] Worster (1996), 406.

How can science (a highly rational project) come to understand the workings of a chaotic ecosystem? Rational thought cannot, through itself, come to understand the chaotic or irrational, and, yet, this is exactly where science has taken ecology. Perhaps, randomness only appears as silence or a gap that science cannot articulate (as in Foucault's archaeology of madness as silence), although even this has problems.[38] If randomness can only exist in silence, then ecologists who claim to explain it are further off-track than earlier ones who remained silent about it and thought, instead, that nature was ordered. As a consequence, science is not simply progressive in any linear manner.[39] When science turns back on itself and expresses perspectives and ideas that invalidate themselves (for example, when, logically, we need to admit that ecosystems are not governed exclusively by logic) researchers become engaged in a paradox. Scientists cannot translate across the boundary between logic and illogic. They had reached the limit of science. These are the paradoxes that ecology encountered when it began to address issues of randomness and chaos.

Until now, I have discussed randomness, chaos, and stochasticity interchangeably, because, until the advent of theories and models that addressed these concepts, the distinctions between them were not important. As the 1970s continued, ecologists found it became more and more difficult to treat these concepts as interchangeably. The greater understanding of these concepts that ecology offers contains new avenues for studying nature (avenues that have not been adopted or even investigated in outdoor recreation).

---

[38] Foucault (1988). See Derrida (1978a) for a critique of Foucault.

[39] However, at the same time we must ask whether it is actually more progressive to admit the a-progressiveness of science.

Chaos Models

> *There was a human being who was born, lived, and then, by some means or another, died. There. You may fill in the details from your own experience. As unoriginal as any other tale, as unique as any other life.... A life that is, like any other, unlike any other. ~ Neil Gaiman*[40]

In the early 1980s, ecologists began to question the assumptions that deterministic models were based on. I argue that, in addition to more powerful technologies, ecologists were encouraged to examine alternatives because of the role that extradiscursive elements had in bringing the limitations of traditional models to the fore. Many of the early studies of chaos, for example, began with a surprise. Early researchers, focusing on single species in simple time discrete situations, were surprised to find these systems behaved unpredictably. In addition, "one of the surprises of nonlinear dynamics is a kind of universality to chaos: dynamical features present in the simplest models appear in a wide variety of more complex models."[41] These changes resulted from developments in technology and theory, certainly, but also they arose because a nonhuman entity asserted itself in surprising but consistent ways.

Until the advent of chaos models, ecologists thought that populations, for instance, were governed by simple forces and the models assumed that future changes would be patterned on these simple forces. In other words, ecologists saw governing forces as deterministic of future changes. In the late 1970s, ecologists began testing this assumption. It quickly became apparent that the governing forces of ecosystems were far more complex than they thought. These realisations and questions necessitated an examination of models of chaos that were being developed in other disciplines. As ecologists did so, they realised that "[t]he best definition of chaos is that chaos is a sensitive dependence on initial conditions (and not just for some special initial conditions)."[42] If initial conditions change, the resulting effect would, over time, become more and more divergent. In other words, a small change in initial conditions results in significant and growing amounts of deviation as time passes. Since there is often no way to predict the initial conditions with complete certainty or the timing of any changes to them, the results appear chaotic.

---

[40] Gaiman (2001), 322-323.
[41] Hastings et al. (1993), 9.

In systems theory governing forces are called attractors.

> Loosely…, an attractor is something which attracts initial conditions from a region surrounding it called the attractor's basin of attraction. Familiar examples are stable equilibria (point attractors) and stable limit cycles (periodic attractors). More recently, attention has been focused on low-dimensional 'strange attractors,' which exhibit highly irregular or 'chaotic' motion.[43]

In the case two of simple divergent variables, each variable is pulled toward an attractor and away from the other variable. In this case, reversal works to return the system to its original state (provided a steady-state divergence). With strange attractors, however, the system does not diverge steadily. Variables do not remain constant in their relationship to each other; they do not simply diverge or converge. Changes in the relationship between attractor and variable occur throughout the sequence.

---

[42] Hastings et al. (1993), 2.
[43] Schaffer (1985), 93.

Thus, when dealing with chaotic systems, the variance over time between trajectories of variables becomes more important. This variance is called the Lyapunov exponent and is "defined as the average rate of trajectory divergence *caused by the endogenous component*."[44] Lyapunov exponents are a measure of stability inasmuch as they indicate the constancy of the rate of divergence between system variables. Given enough inconstancy, the system will collapse. Measuring the changes in trajectories renders Lyapunov exponents for any system. When one considers "two trajectories that start near each other and are affected by the *identical* sequence of random shocks," one obtains a measure of the Lyapunov exponent for a chaotic system in particular.[45] In the end, Lyapunov exponents help distinguish between a dynamical system "characterized by either stability of trajectories…, or by sensitive dependence on initial conditions."[46]

Although both chaos and stochastic forces cause change, chaotic dynamics can be distinguished from stochastic ones: random changes cannot be predicted, but chaotic one can be, up to a certain point. Schaffer, for instance, suggested that prediction is one criterion for doing so: "[i]f ecological attractors turn out to be 'strange,' … longrange prediction is out of the question because of the uncertainty surrounding any determination of the state of the system (experimental error). However, shortterm prediction, e.g., from one maximum to the next, should still be possible."[47] In terms of chaotic systems, then, "predictions can be made over short time scales because the dynamics are deterministic. However, predictions cannot be made over a long time scale. This inability to predict over the long term is the hallmark of chaos."[48] When determining whether a change is chaotic or stochastic, the ability to predict future states becomes central. Unfortunately, this ability does not distinguish noise. Outside interference, noise, can change a system's state; ecologists needed a way to distinguish one type of change from the other. Noise differs from chaos because it is exogenous to the system, which means that "[n]oisy stable dynamics lack predictability at any time scale."[49] Chaos, along with noise, are both unpredictable at longer time scales, but noise is never part of the system parameters as is chaos.

---

[44] (Turchin 1993), 171. The cause must be endogenous because otherwise we are not getting a measure of the system's inherent level of stability.
[45] (Turchin 1993), 171.
[46] (Turchin 1993), 171.
[47] Schaffer (1985), 103.

Finally, when ecologists speak of chaos and noise, they also distinguish these from error. The simplest kind of error is observational error, which "does not introduce any new interesting effects in a dynamical system."[50] Measurement and observational error are both important, but neither of them directly affects the dynamics of the system under investigation. Because chaos is defined as a sensitive dependence on initial conditions, errors in measurement of those conditions cause the appearance of chaos when none is actually operating. Furthermore, low levels of chaos combined with small errors can mimic higher levels of chaos. In terms of noise and stochasticity, ecologists soon realised that an important distinguishing feature was whether or not the disturbance was internal or external to the system. The origin of error, on the other hand, turned out to be less important than its effects when determining its significance.

> Even small measurement errors can make a deterministic chaotic series highly unpredictable and effectively stochastic…. Moreover, an estimate of predictability per se says little unless there is some means of determining whether the cause of unpredictability is external [that is,

---

[48] Hastings et al. (1993), 3.
[49] Turchin (1993), 171.
[50] Serletis, Shahmoradi, and Serletis (2007), 884.

> noise] or internal to the system, and if internal, whether it is due mainly to low-dimensional chaos plus measurement error.[51]

Many other complications arose as ecologists tried to identify noise and error, but no matter how they operationalize these terms, the problem of distinguishing chaos, noise, and error from each other remains formidable.

As formidable as these challenges are, it now appears clear that noise, error, stochasticity and chaos must be differentiated and included in studies of natural systems. Ecologists have, since the 1970s, been attempting such a task. One of the things they discovered is that "[c]haos can be detected using several approaches…, [but] visual inspection of time series is not a reliable method for detecting chaos."[52] The most common method used in OR to determine the health of an ecosystem or the impacts users have on specific areas is visual counts of species and individuals (recall Monz's study on the effects of trampling on arctic plants discussed above[53]). As a result, outdoor recreation researchers cannot, because of methodological limitations, reliably determine whether a system is chaotic or not. They cannot distinguish between noise, error, and chaos, and, thus, cannot identify any of the interactions between and among them. Moreover, OR studies often attempt to ascertain the health of a system or portion thereof. In most cases, OR assumes that stable systems are healthier because they will persist. This assumption, in turn, is based on the idea that natural systems, because they have persisted so long, must be stable.

Because of these assumptions, OR incorrectly links health exclusively to non-chaotic dynamics. This linking is incorrect for a couple of reasons: first, because chaotic dynamics are not, perhaps contrary to initial intuition, completely unpredictable, they have a level of persistence and stability. Second, chaotic dynamics often characterize those systems OR researchers use as the yardstick to measure other systems against. OR sees non-chaotic systems as healthy, in part, because it cannot see the chaos already present in the systems it uses as yardsticks against which to judge the health of other systems. In other words, chaos can be an important part of healthy systems, but OR's methodological structure prohibits a discussion of this possibility because the techniques used to gather the data are too coarse to differentiate among the relevant factors. The result is that chaos assumes a negative or unhealthy connotation. Furthermore, OR may even fail to see the very damages it is hoping to prevent or repair.

---

[51] Hastings et al. (1993), 19.
[52] Hastings et al. (1993), 7. See also Dufrene and Legendre (1997).
[53] Monz (2002), discussed above on page 301.

Regarding the last point, it is ironic that the emphasis OR places on stability as an indicator of the health of natural areas actually makes it harder to see instances when stability is not representative of the health of an area. A particular stand of vegetation, for instance, might undergo stochastic or chaotic changes, but, through management policies (for example, fire suppression) and/or minimum impact practices (for example, camping in the forest instead of the lakeshore), these changes are vilified and minimized or eliminated. For instance, Christensen and Cole have noted that "there are reasons to expect that proximity to lakeshores is poorly correlated with impact potential, particularly soil and vegetation damage." Moreover, the policy of sleeping two hundred feet from shorelines, which is touted as reducing impacts, could actually have negative impacts in the sense of altering the functioning of chaotic dynamics in the forest; however, the discursive structure of OR makes this possibility much harder to see.[54]

---

[54] Christensen and Cole (2000), 77. They also remarked that "[e]mpirical data have shown that campsites close to lakes are not more highly impacted than camps away from lakes" (84).

If chaos is actually part of many propositional configurations in natural nature, then OR has an opportunity to alter how it thinks about chaos and change, particularly in the sense of human-induced change. As long as OR continues to frame human interaction with the land in terms of noise or stochasticity, chaotic effects will always be seen as disturbances or disruptions to the system's performance. However, noise can be misinterpreted as chaos and vice versa, which means that what OR thought was noise might actually be chaotic dynamics and, thus, endogenous to the system. It is possible that some of the observed changes in natural nature that have been deemed negative are actually chaotic and part of the normal functioning of that system. Furthermore, when human actions are always classified as exogenous to natural nature, the potential for seeing them as endogenous is eliminated. However, OR's methodologies are not sufficiently fine-grained enough to be able to ascertain whether human action is chaotic or not (and therefore whether detrimental or not). Without the addition of chaotic dynamics, OR studies will continue, in some instances, to fail to differentiate exogenous versus endogenous as well as negative versus positive change.

Although visual counts of time series data are not reliable methods for determining chaos, they can be linked to chaos. For instance, "graphs of population densities versus time in chaotic systems appear to lack a perceptible pattern. This has lead to the origin of an 'intuitive, but technically vague' definition of chaos that refers to the presence of irregular oscillations."[55] Even when we cannot determine for certain the cause of this lack of pattern, chaotic dynamics are one possible explanation that should be investigated. OR studies often use similar graphs to chart changes in plant density, height, type, and distribution (all based on visual counts). So, OR could use the presence of irregular oscillations in the graphs to indicate the possible presence of chaotic dynamics. In the end, however, OR scholars should remember that "visual methods can be misleading and may be insufficient to distinguish between chaos and stochasticity."[56]

---

[55] Hastings et al. (1993), 7.

Chaotic and stochastic systems are essentially non-linear: "[l]inear models yield exponential growth whereas nonlinear models have the potential for more complex dynamical behaviors including periodic and aperiodic cycles and chaos."[57] There has been a need, recognized in ecology for a number of years now, for models and theories that address non-linear system behaviour in nature. "Ecologists are comfortable with the assertion that nonlinear systems can display limit cycles, multiple attractors, and strange attractors, because the assertion is a mathematical fact. Ecologists for the most part also agree that ecological relationships are frequently, perhaps largely, nonlinear."[58] Concepts such as chaos, stochasticity, error and noise all complicate systems behaviours beyond that which OR models can effectively perceive or explain. Dennis, Desharais, Cushing and Costantino summarize how many researchers (whether in ecology or outdoor recreation) must feel:

> [e]cological systems are complex: alternate explanations abound; data collection is challenging; noise is prevalent. Are periodic fluctuations observed in a system really limit cycles caused by some intrinsic, identifiable mechanisms, or are they caused by some unobserved external forces? Did a system get pushed into a different attractor, or have conditions assumed by the model simply changed? Is it chaos or is it noise?[59]

---

[56] Hastings et al. (1993), 7.
[57] Dennis et al. (1995), 261.
[58] Dennis et al. (1995), 279.
[59] Dennis et al. (1995), 279.

These questions reveal the complexity and difficulties associated with newer ecological modelling. OR, perhaps understandably, has not waded into this messy soup of models, concepts, and theories. The discursive practices in OR that channel research away from concepts such as chaos and randomness are not set, however. The inclusion of these concepts and models in OR is, therefore, possible. Even so, productive research in OR can be and is being done without using chaos models. I would encourage OR scholars, nevertheless, to recognize that discursive practices funnel research in certain directions and to explore the implications of this.

In addition to raising questions about the stability and predictability of natural systems, models dealing with chaos and stochastic forces also led ecologists to develop models of patch-dynamics and hierarchy theory. For example, one type of chaos model assumed that stochastic dominance occurred and extinctions followed; however, the extinctions were at a smaller scale than the spatial distribution of the populations. Thus, one subgroup may become extinct, but it could be re-colonized later by another subgroup. What was chaotic or stochastic in one cell or patch (as these subgroups became known as) might not continue to be so at a higher level of organization. Recognitions such as this gave rise to what has been called landscape ecology. This perspective in ecology recognizes numerous levels of patch organization from the individual microscopic cell up to the entire known universe.

Landscape Ecology: Patch-Dynamics Models

> *The concepts 'individual' and 'species' equally false and merely apparent.*
> *'Species' expresses only the fact that an abundance of similar creatures appear*
> *at the same time and that the tempo of their further growth and change is*
> *for a long time slowed down, so actual small continuations and increases*
> *are not very much noticed (—a phase of evolution in which the evolution*
> *is not visible, so an equilibrium seems to have been attained, making possible*
> *the false notion that a goal has been attained—and that evolution has a goal—)*
> *~ Friedrich Nietzsche*[60]

At the same time as ecologists were investigating chaos theory, complexity theories were becoming more popular. In order to understand ecosystems, according to complexity theory, ecologists needed to viewed them on a number of different levels of organisation: "[a] patch at a given scale has an internal structure that is a reflection of patchiness at finer scales, and the mosaic containing that patch has a structure that is determined by patchiness at broader scales."[61] In order to understand any system then, the level above and below the focal needed to be included. Thus, the ideas of patch dynamics and hierarchy theory arose: "[p]atch dynamics as a concept has been widely used in population and community ecology and provided new insights into problems of population dynamics and persistence, community structure and stability, and landscape dynamics since the early 1970s."[62]

Throughout the 1970s and 1980s, more ecologists were looking for and discovering complications, variations, and deviations from earlier models. These ecologists began to argue that, rather than being homogeneous, (eco)systems display a significant amount of variation across space. This variation is especially evident when moving from one cluster of individual elements to another. The discursive practices surrounding ecology help us to understand how all this variation and difference went unnoticed before or was downplayed as insignificant. The propositional configuration that existed before the 1970s shed a number of collaborators and gained some new ones as spatial variation became a more central feature in ecology. While the overall propositional configuration changed, some elements in it remained constant, and existence applies only to the configuration, not the individual elements. Thus, to say that spatial variation existed all along, and ecologists only discovered it in the 1970s, is to give existence to parts of the configuration instead of the whole. It is more accurate to claim that certain aspects are similar between the pre- and post-spatial variation models, but other elements are not. The net result is that what exists also changes. Furthermore, the configuration that came to exist in the 1970s had a past that extended backwards beyond its birth such that we can now say nature contained significant spatial variation in, for example, the 1950s as well.

---

[60] Nietzsche (1967 [1901]), 282.
[61] Kotliar and Wiens (1990), 254.
[62] Wu and Levin (1994), 461. See also Chesson (1981) and Pickett and Thompson (1978) for early examples and Forbes, Ebersole, and Strandberg (2001) for a more recent one.

Ecologists have long laboured under the assumption that there was a balance to nature and turned most of their efforts toward discovering this balance. However, this is an example where the extradiscursive aspect challenged ecological models. While looking for a harmoniously balanced nature, ecologists were surprised by some of their findings. They often made comments to the effect that their study 'contradicted earlier work,' or was 'in contrast to other studies.' Thus, surprise becomes an important indicator of the diversity of actants in the process of real-*ising* our world. The extradiscursive element surprises ecologists and demands that they take spatial heterogeneity into account.[63] As a result, models such as patch dynamics now address the prevalence and significance of spatial heterogeneity. As Levin noted,

> [v]irtually every population will exhibit patchiness and variability on a range of spatial and temporal scales, so that the definition of commonness or rarity is scale dependent.... Virtually every ecosystem will exhibit patchiness and variability on a range of spatial, temporal, and organizational scales, with substantial interaction with other systems and influence on local stochastic events. These phenomena are critical for the maintenance of most species.[64]

Models that sought to explain spatial heterogeneity are most often grouped together in the sub-field of landscape ecology: "[a] landscape is a kilometers-wide area where a cluster of interacting stands or ecosystems is repeated in similar form. The landscape is formed by two mechanisms operating together within its boundary—specific geomorphological processes and specific disturbances of the component stands."[65] These various stands are also called patches and can be composed of populations of plants, animals, or interacting communities of the two. "In simplest terms, patches are communities or species assemblages surrounded by a matrix with a dissimilar community structure or composition."[66] There are many different understandings of patchiness and spatial patchiness is not a uniform concept: what is seen as a patch to a gopher may not be to a bird or a plant. As Wu and Loucks note,

> [s]patial patchiness includes both physical (or abiotic environmental) and biological aspects, which are interactive and interwoven across spatiotemporal and organizational scales. Biological patchiness occurs at both primary producer and consumer levels. Vegetation patterns present the most conspicuous spatial patchiness and provide a framework for patchiness at consumer levels in terrestrial systems.... Different causes

---

[63] Swinnerton (1999), quoting Spirn, noted this too when he claimed that "failure to appreciate the dynamic autonomous role of nonhuman features and phenomena promotes the illusion that humans can construct and control everything" (203).
[64] Levin (1992), 1960.
[65] Forman and Godron (1981), 733.
[66] Forman and Godron (1981), 734.

> and mechanisms operate on different spatial, temporal and organizational scales, and create a hierarchical structure of patchiness.[67]

In patch-dynamics models, disturbance becomes central as it is one of the main forces that create and distribute patches within a landscape: "[t]he origins of patches differ according to the disturbance regime in the patch, disturbance in the matrix, natural distribution of environmental resources, species introduction by people, and time. These differences in patch origin determine the species dynamics and the stability and turnover of patches themselves."[68]

There are many characteristics of patches that have been assuming greater and greater importance over the years. In addition to considering number and location of patches, ecologists have also been considering edge effects, because the dynamics at the centre of the patch may well be different from those at the edge of the same patch. The shape of the patch has also turned out to be more significant than originally thought. Different patch shape characteristics (especially width) give rise to different dynamics. Proportion of the edge to the interior is also an important factor in understanding patch dynamics. Some patch shapes are particularly important: ring patches and peninsula patches, for instance have profound effects on species composition. Corridors that connect one patch to another are yet another important feature of patches.[69] These aspects, grouped all together, are called within-patch dynamics.

In addition to within-patch dynamics, there are also between-patch dynamics. Some of the factors to consider here include species rain (the amount of mobility of species between patches in a landscape), aggregation ("[t]he spatial distribution or dispersion of patches"[70]), and contrast ("[t]he degree of difference between patches or between patch and matrix"[71]). These factors influence the type of distributions within and between patches at any given level in a landscape. Components within a low-aggregation patch, for instance, will be nearly randomly distributed; components within a low-contrast patch will more closely resemble each other.

---

[67] Wu and Loucks (1995), 447.

[68] Forman and Godron (1981), 738.

[69] See Forman and Godron (1981) for a good introduction to these concepts.

Patches and the dynamics within and between them form one half of patch-dynamic models. The other half is comprised of the hierarchy of the patches. In any given area, a patch hierarchy ranges from the smallest element (called the grain) to the largest (called the extent). As mentioned above, patch dynamics should be studied at a number of levels in order to gain a more comprehensive picture of the complexity of the system. "By ignoring the position of a patch in a hierarchy, one averages away heterogeneity at lower and higher levels to which an organism may indeed respond."[72] Indeed, this is what many pre-1970s ecological studies did and what many OR ones do, which is unfortunate because

> one of the most significant contributions … [of] hierarchy theory … has been to enhance the awareness of scale and facilitate operational measures of scale across a wide range of disciplines…. Recently, hierarchy theory has emerged as a conceptual framework that fosters new approaches to ecological studies, ranging from population regulation to landscape dynamics…. While it seems that the patch dynamics perspective has emerged without a clear contribution from or to hierarchy theory…, we argue … that the integration of the two has led to what may be called an emerging paradigm with new insights into the complexity and stability of ecosystems.[73]

Robust understandings of complexity and stability are sacrificed in OR because most studies examine one or two species (interacting or not) in a given area and usually at only one or two levels of organisation. For example, campsite studies that determine the level of impact often use visual counts to establish the amount of impact human use has on campsite vegetation. Studies such as these examine patch dynamics only at the campsite scale. Within-patch dynamics have been considered in terms of the shape of the backcountry trails and the implications these dynamics have for conservation and protection. Hammitt and Cole, for example, studied shape, but did not extend consideration to any other within-patch dynamic.[74] OR discourse has rarely incorporated many patch dynamics, such as within-patch distribution patterns, edge effects, species rain, or aggregation levels.

---

[70] Kotliar and Wiens (1990), 254.
[71] Kotliar and Wiens (1990), 254.
[72] Kotliar and Wiens (1990), 258.
[73] Wu and Loucks (1995), 450.

One of the major considerations in hierarchical patch dynamics is the relationship between pattern and process. Processes can create, maintain, modify, and destroy patterns, while patterns can either facilitate or constrain ecological processes.

> Examples of ecological processes are numerous … [and] are responsible for the formation of patterns, and for determining the dynamics of types of patchiness…. On the other hand, spatial patchiness imposes structural constraints on ecological processes operating at different organizational levels. Thus, one should study ecological processes in their context and search for patterns based on underlying processes.[75]

The relationship between processes and patterns seems to be a limiting one. In other words, the choice of patterns is not unbounded. A number of analyses have shown what could be called 'breaking' or 'tipping' points that mark the boundaries of the 'domains of scale.' For example, "spatial variance can exhibit a staircase-like (or step-wise) pattern of change when obtained over a wide range of scales."[76] As a result, scales themselves can be grouped into levels at which the types of responses to processes are similar. Once that boundary is crossed, a new type of pattern appears. Prior to crossing the boundary, the patterns changed but stayed within a common type. Patch dynamics in ecology seems to suggest that there is a pattern to structure, which is similar to Derrida's concept of structurality, which also indicates that there is a pattern to structure. Part of this pattern is a repeating paradox whereby self-contradictory elements exist. Processes lead to pattern changes and indicate instability, but, at another level, there are patterns to this instability. A type of ordered randomness appears.

---

[74] Hammitt and Cole (1987).
[75] Wu and Loucks (1995), 451-452.
[76] Wu and Loucks (1995), 452.

Patch hierarchy models can also accommodate chaos models because chaotic or stochastic forces can dominate at certain levels in the hierarchies, but not others: "ecological systems at small spatial scales can be treated as … ephemeral systems with no equilibrium properties."[77] Finally, there are two important interrelated components in patch dynamics: the principle of incorporation and the concept of metastability. We have already seen that lower-level chaotic or stochastic processes are an integral part of higher-level structure and processes (patch dynamics interact with the matrix to produce bounded higher level patterns and processes). Thus, nonequilibrium patch processes at one level can be incorporated into a type of equilibrium state at a higher level. This quasi-equilibrium state has been termed metastability and illustrates how order can arise out of apparently random fluctuation. With metastability,

> nonequilibrium dynamics at one scale can become the means of quasi-equilibrium at a higher level (either spatially larger or temporally longer). For example, single tree-falls induce local gap dynamics that create nonequilibrium outcomes at the gap level; however, gaps are incorporated readily as an area-wide mean process at a larger forest stand level, leading to a 'shifting mosaic steady-state'…. A large blowdown cannot be incorporated in forest stand dynamics, but it can be at a landscape of larger scale.[78]

---

[77] Wu and Loucks (1995), 452-453.
[78] Wu and Loucks (1995), 453.

Patch-dynamics hierarchies and chaos/stochastic modelling relate local dynamics to larger scale phenomena and integrate information across levels in ecological systems.

In the end, it seems that "[t]he 'rules of the game' for ecological system stability can change drastically across scales of time, space, and organization. Thus, the predictability of ecological systems also may depend on scale, as well as the nature of the pattern and processes under consideration."[79] As ecologists studied patches, they discovered a substantial amount of diversity and complexity. However, the rules of the game change dramatically with each level of complexity. This level of change must be distinguished from earlier models of change. Ecologists are now saying that not only does nature change appearances at different levels, but also the processes which generate those observable changes also change and, in fact, some of the effects of these changes in processes are not seen at the level of organisation that the change took place in. For example, the changes that give rise to metastability do not operate at the level of the metastability. In fact, when these changes are examined at the level in which they operate, ecologists find them to be disruptive instead of stabilizing.

Some of the findings of patch dynamics could be useful in OR studies. The paradoxes between chaos and order, randomness and patterns, and disruption and stability do not need to be resolved in a definitive manner. Patch-dynamic models and chaos theory can admit that both stability and instability or chaos and order exist in any given system. This admission would allow OR to recognise chaos at one level (say, a campsite level) and not at the larger level (say, the forest stand or valley). If OR were to recognize that chaos and order characterise more complex systems, then perhaps more flexible management practices and minimum impact policies could be developed. Currently, most OR impact and management studies are directed at the elimination or containment of all signs of human presence because that presence causes chaotic or disruptive effects. If chaos is not automatically unnatural, then human presence is not automatically unnatural just because it produces chaotic effects. However, the reverse is equally the case: just because human actions may be chaotic does not make all our actions acceptable in the wilderness. The challenge in OR, then, is to address this complexity without undermining the argument for preserving wilderness in Canada.

---

[79] Wu and Loucks (1995), 459.

Patch dynamics offer one way to address the paradox of natural nature. When chaotic dynamics are found to be prevalent at one level, it is possible that this can be absorbed at another level. Of course, we also have to recognise the possibility of the opposite happening: stability at one level can give way to chaos at other levels. We might call this the decomposition of structure, which can be linked to Derrida's notion of structurality. If the nature of structure is that it is both random and patterned, both stable and unstable, then ecologists and OR scholars can engage the paradox they have up until now been unable to examine. The potential is now available to understand how self-contradictory elements in propositional configurations can co-exist. Nonetheless, we must realise that this depends on a decision we make about how to view these systems. We see either chaos or order depending on the level we choose for analysis. Our input is one necessary ingredient in how ecosystems appear. Thus, the resolution of the paradox of chaos in nature does not simply happen; it is not more objectively true to view an ecosystem in one way or another; it is not inevitable. Ethics and politics now become important factors. There is no necessary reason for adopting any particular view of stability or randomness/chaos. We must keep in mind that we choose the level and there are effects to which we must remain accountable.

Expanded Modelling: Human Thought In/And Nature

'Truth' is therefore not something there, that might be found or discovered—
but something that must be created and that gives name to a process,
or rather to a will to overcome that has in itself no end—
introducing truth, as a processus in infinitum, *an active determining—*
not a becoming-conscious of something that is in itself firm and determined.
It is a word for the 'will to power'
~ Friedrich Nietzsche[80]

The realisation that our modelling is implicated in how ecological systems appear to us opens up new avenues for exploration. If we choose how to measure an ecosystem, and that is an important element in how it presents itself, then we cannot continue to think of ecosystems as something separate to which our models merely refer. Our descriptions of propositional configurations become part of the measurement apparatus. In some cases, we can make complementary measurements, but never at the same time on the same system, because different and incompatible modelling apparatuses are required. Choices must be made, and these choices affect the outcome of how natural nature appears to us. The question now becomes how to incorporate our thinking as one more link in a propositional configuration that is presenting itself for membership in the collective world we share.

Hierarchy theory can be expended in an attempt to address this question: perhaps there is another level that has yet to be included. For instance, if an ecologist or OR researcher conceives of the whole protected area in which a number of campsites fall as the last level in the hierarchy, another level could still be added to this hierarchy, one that reflects the realisation that our very thinking and modelling influences the way this proposition presents itself to us (even to the point of questioning whether there is a 'nature' distinct from us). Adding this type of level to hierarchy theory represents a step up the levels of learning that Gregory Bateson has suggested. According to Daniel White, Bateson called level one learning the simple learning of a task. Level two learning means learning to learn, or becoming more effective at learning tasks. Level two learning can also mean developing the ability to learn different types of tasks. Level three learning is learning how to learn to learn. In other words, we develop propensities to approach learning in certain ways; character and culture are established at this level. The final level of Bateson's hierarchy is level four learning: learning how to learn how to learn how to learn. It is at this level that learning becomes meta-individualistic. Evolution occurs at this level. The recognition that humans and the environment are in a relationship with each other and it is this relationship, not the constituent components of it, that evolves is itself an evolutionary step from earlier views on ecology.[81]

---

[80] Nietzsche (1967 [1901]), 298.

White has explained these levels in relationship to ecology and suggested that when humans see themselves as the highest category capable of learning, they reverse the logic of evolution: "the organism, 'man,' historically a subsystem, attempts to maintain 'his' stability and survival by creating changes in the larger system, subordinating it to his designs … The ecological crisis is a predictable result."[82] Contemporary ecological theory works on this assumption that humans learn and in turn create (control) culture and the larger level systems. The corrective view that White asserts reverses this logic. "The result is that evolution produces organisms capable of developing language and culture, which in turn allows the formation of families, societies, and individual 'characters,' which in turn allows simple conditioning."[83] The highest order here is the supersystem, which evolves, and within it various component parts develop and adapt (for example, thinking humans). "The hierarchic relation between subsystem and supersystem, with the supersystem intact, is presupposed in the schema … it is the *a priori* condition of complex systems."[84] Therefore, for humans to exist, there must exist, *a priori,* a supersystemic 'mind' within which we develop. This supersystem is not a result of what we do on this planet: we are the result of its evolutions. As Lemke noted, "[l]ife did not begin with micro-organisms that eventually got together to form ecosystems that eventually united into [a supersystem]. There was always [this supersystem], even prebiotically."[85]

---

[81] White (1998), 110-112.
[82] White (1998), 121.

Patch hierarchy theory contains hints of White's view. Movement up the hierarchy increases the scope from an individual cell to the entire planet. Since, obviously, humans are part of the planet, when we say that the ecosphere evolves, the constituent parts must likewise adapt to these changes. In other words, humanity developed ecological thinking within another system, as a result of that system, and, thus, that very thinking is subjected to the evolution of the super-organismic level. It is a mistake to read changes in ecological modelling as independent from or occurring without the influence of the larger supersystem. Over time, this supersystem exerted an influence on our conceptualization of humans as separate from natural nature that challenged conventional models. As a result, newer models were developed in ecology that contain the potential for seeing humanity in a different relationship with the environment. This process of evolution can be thought of as a form of communication or différance between various component parts of the supersystem (in this case between natural nature and ecologists and OR researchers). This communication encourages ecologists and OR scholars to see themselves as part of the system that learns and changes. Communication, often in the form of traces and différance, between these links in the propositional configuration is represented as an evolution in the ecosphere not in ecological science or OR scholarship alone.

---

[83] White (1998), 122.
[84] White (1998), 123.
[85] Lemke quoted in White (1998), 122.

The pattern of thinking that humans can and should be separated from nature is a product of the supersystem's evolution not ours alone, and, thus far, it has turned out to be an unsustainable pattern. As we can see in recent developments in ecological theory, the supersystem is beginning to exert an influence to correct this pattern. This correction is complex and more a process than a product. That is, there are not only different actants, but also there are different types of actants involved in an ongoing process of convoking our collective. The propositional configurations that may be emerging in ecology today contain various elements that exert their own influence on the process of emergence. Thus, scientists, discourse, and ecological systems with their constituent parts are all implicated in real-*ising* our collective world. For example, scientists propose a new model for understanding or predicting something in the world, and the world then responds and interacts with the scientist and the measurement apparatus.[86] Throughout this entire process, discourse is operating. Discursive regulations, restrictions, productive possibilities and opportunities, and governing dynamics are all operating on the various actants according to the power-knowledge regime within which science and society are enmeshed.

---

[86] Of course, this process works the other direction as well: various artefacts act in surprising ways that scientists then interact with as they develop new models.

The development of chaos models and patch-dynamic hierarchies are suggestive of how the process of real-*ising* the collective functions. Elements that were previously excluded in ecological thought began to kick back and scientists attempted to take this into account. The traces and différance, hauntings, and undecidability of paradoxes that I discussed in Chapter Five are examples of how these extradiscursive elements manifest in both ecology and OR. Chaos models, for instance, addressed some of the difficulties that stability models encountered. Nature, it seemed, was not acting in accordance with stability or simple deterministic models. Chaos models can be thought of as a new proposal for our collective put forth by different collaborators in terms of the relevant discursive (pre)conditions. It remains to be seen whether or how completely this new proposition will be accepted into our collective. As with all propositional configurations, continual work is required to maintain its existence. Existence is now a process rather than a state; it is achieved, constantly, by the work the various collaborators do. I am claiming that understanding the traces and différance, hauntings, and undecidability of paradoxes in both ecology and OR might prompt an expansion of each discipline to include humans and their thinking as integral components in how natural nature appears and functions.

The changes in ecological modelling that began in the late 1970s can be read as the supersystem's influence in altering the conditions of our thinking such that ecologists could conceptualise nature in an ever increasingly complex hierarchy of patches (some of which were chaotic and/or random). I suggest that we continue this trend and include our selves, our modelling practices, and the role discourse plays. We are implicated in what we see and, therefore, we must take into account our own thinking when trying to describe ecological systems because both these systems and our selves are part of a larger system.

Paradoxical Ecology and Outdoor Recreation

> *The lesson of quantum mechanics and even of classical physics…is that nothing is determined, nothing is certain, nothing is completely predictable; there are only propensities for certain things to occur. ~ Karl Popper.*

The surprise that researchers often express indicates that ecological systems are not mute and passive objects awaiting discovery. If ecological systems are propositional configurations that have extradiscursive links, then these links make themselves heard in ways that ask to be taken into account. For instance, Friederichs noted that it is not always the case that an animal population makes use of the niches supplied by the environment. In some cases, the animals actually make the niche. Crows on the islets of the North-Sea that build their nests on the ground instead of trees are an example of this behaviour: "this behavior may be, as it were, an invention, an intellectual action of these clever birds. Thus they have *made* the islet a niche for themselves."[87] For this reason, ecological systems are propositional configurations within which different elements help to propose the configuration as a member of our collective. The crows in Friederichs' study were active in putting forth the niche as a slightly different proposal than previous understandings had it.

As another example, Kavanau studied mice in the laboratory and found that they "tend to react to the arbitrary imposition of a regime by opposition to it; if the animals have the power to counteract the effects of nonvolitional modifications, they do so promptly."[88] Here the very ecological science that takes mice as objects makes a potentially productive move, turning back on itself. How can a mere object exercise volitional control or oppose the arbitrary imposition of a regime? Why are humans the only actors in these scenarios? No one disputes whether scientists are actors, but science is now raising challenges to the idea that mice (and indeed all elements in ecological systems) can be limited to only the status of an object. The mice act more like Pasteur's lactic acid ferment yeast; they are actants not objects.[89]

---

[87] Friederichs (1958), 156.

More recently, Chesson investigated deterministic versus indeterministic population models and was surprised to find that the deterministic "model fail[ed] to describe accurately the average population and spatial variation. In addition, it [gave] a different answer than the stochastic model for the mean population on a particular patch."[90] Here is another instance where being attentive to the requests of the various actants opens new avenues for exploration. Chesson's article appeared in the early 1980s, when studies of patch dynamics were only just beginning. At this point, Chesson indicated his surprise that populations did not behave in the way standard models predicted. The proposition of patch dynamics had not yet been realised. At this point, it had a tenuous existence that consisted largely of an emerging consistency in the behaviour of populations, the various technologies and techniques used to study populations, and the surprise and curiosity in Chesson. Over time, this proposition became more and more elaborate as new collaborators combined to more clearly articulate this proposition.

---

[88] Kavanau (1966), 114.
[89] This example of lactic acid ferment yeast was discussed in Chapter Six. It refers to the process by which an investigator and an entity combine to produce knowledge in the laboratory.
[90] Chesson (1981), 308.

A similar process of surprise and curiosity related to population dynamics could occur in OR. However, to date, most of the models OR uses to determine impacts and management policies are deterministic. Thus, according to Chesson, these policies may inaccurately depict average population size and distribution. Adopting chaos theory in OR complicates matters greatly. Such a switch would mean re-evaluating management practices and impact studies in light of the shifting and somewhat unpredictable behaviour of ecological systems. This re-evaluation, however, cannot be seriously undertaken without a deeper understanding of the models in ecology. The commitment to older models based on steady-state or equilibrium systems precludes alternative visions of how wilderness areas function. Newer ecological theory, while much more complex, represents an improvement over older models that fail to accurately describe the average population and spatial variation.

In order to explore the possibility of contemporary ecological theory, OR would need not only to grapple with the more complicated ecological theory, but also to undertake the challenging task of working that theory into management practices and minimum impact studies.[91] Although challenging, such an undertaking could prove very valuable if it results in a greater understanding of the operation of natural systems and how our actions influence them.

Newer ecological modelling, because of the inherent paradoxical nature of many of its concepts, is better positioned to address the role actants play in real-*ising* our collective world. When these elements kick back at ecologists and OR researchers, they ask to be accounted for in certain ways. Consequently, our modelling must change. Furthermore, attention to paradoxes highlights their never-ending movement of il/logic. Paradoxes cannot be treated in any simplistic manner (and this includes simply treating them as a logical problem to be solved). The dynamics of paradoxes require an explicit account of our departure point from the merry-go-round spinning of paradoxical processes. If the play of différance is important (note that this is the departure point from the paradox), then keeping this movement alive and in play, that is, increasing différance or the playing effect of difference, would open space for various actants to speak. A sensitivity to différance, traces, hauntings, and paradoxes focuses attention on the process by which artefacts become members of our collective.

---

[91] This has already been suggested by some authors. For example, see Gibson et al. (2000) who called for "an integration of ecological concepts (such as theories developed from disturbance ecology) with natural resource management" (230).

Thus, I am encouraging ecology and OR to adopt a will to engage instead of a will to know. The attempt to keep paradoxes in play, even as decisions are made and actions taken, is one example of how a will to engage might change the way ecology and OR produce knowledge. If ecological systems are chaotic, random, patterned, ordered, stable, unstable, that is to say, paradoxical, then a puzzling dilemma arises: how does one choose what management decision to support when the stability of an ecological systems depends on how one looks at it? Take, for example, a forest ecosystem where "one patch of 0.1 ha size may periodically switch from one of the four old-growth species to any of the others, leading to continuous unpredictable change over time. At spatial scales of 1-16 ha, successional direction is somewhat predictable—a mixture of four species results, although the proportions of the four may vary individualistically over time."[92] Depending on the level from which one observes the forest, the results can vary significantly. Ecologists choose the level to observe and could even examine multiple levels at the same time; however, integrating these results into a management policy or minimum impact program is problematic. Should management or minimum impact policies be geared toward keeping the system stable, allowing unpredictable changes to occur, or both?

---

[92] Frelich and Reich (1995), 342.

The will to engage directs attention differently than the will to know. In the above example, the will to know might allow the forest to be stable and unstable; nonetheless, it invests both these interpretations with truth claims that allow only one to manifest at any given level of organisation. Hence, the forest is either stable or unstable, or is stable at one level but not another. Furthermore, this stability and instability will be consistently displayed as long as the same variables are observed at the same level of organisation. Unless other change agents are uncovered, the forest will not switch from stable to unstable.

In outdoor recreation the will to engage complements research that suggests backcountry users should be paying more attention to their surroundings in order to reduce their impacts. Cole, for instance, investigated whether replacing hiking boots with light-soled shoes or sandals had any discernible effects in terms of protecting the vegetation from trampling at campsites. He concluded that the soles of hiking boots had little measurable effect on plants. Even so, Cole did note that "an effective way to minimize trampling damage to campsite vegetation is to avoid stepping on plants. Campers in light shoes may be more likely to watch were they place their feet than campers in heavy boots. If so, they will cause less vegetation damage."[93] It is a matter of paying more attention and being more fully engaged in the environment. Cole's suggestion works in the opposite direction from many of the current minimum impact practices that insulate users from the environment. Modern low-impact practices are designed to shield nature from all effects of users. Of course, this is not possible in the extreme, but it is the general direction of many policies, rules, and practices (for example, using a stove instead of a fire places users in different relationships with technology, the forest, the weather, and themselves). Conversely, wearing softer shoes, Cole noted, may make users more attuned to the contours of the ground and may, perhaps, direct their attention to the way in which they interact with the environment instead of the sub-textual lesson of minimum impact standards that tells them they do not belong.

A will to engage would also change how ecologists and OR scholars think about knowledge. Instead of believing they could solve one problem (or component of one) and then move on to another, the will to engage requires that ecologists and OR scholars engage in a continual process. By this I mean they respond to a situation and continue to respond to it over and over. Researchers and scientists would enter into a continual conversation or dialogue with the various elements in the situation and, thus, they become another component of the situation. Their responses also become another factor in the circumstances that deserve attention. Propositional configurations exist in varying degrees; thus, what has existence changes as people in ecology and OR respond and interact with natural nature differently.

Finally, a will to engage re-orients matters of implementation. For instance, ecology tells us that ecosystems change (and often unpredictably so); however, minimum impact standards and management perspectives usually construct human influences as negative and attempt to remove or reduce them. As such, there is scant space to discuss whether some human actions could be acceptable or even healthy for natural systems. By remaining more open to paradoxes and contradictions, OR can begin to examine whether all human impacts are indeed negative. Given that one of the goals for outdoor recreation is to connect people to the wilderness,[94] an altered perspective that admits that humans are not necessarily damaging would go a long way toward realising this goal. Perhaps the LNT program could be changed to reflect the uncertainties in ecological knowledge that it is based on. Educational programs, likewise, could train new leaders to be more flexible when it comes to minimum impact practices. As it stands now, the only relationship with nature that outdoor users who comply with minimum impact standards have is an antagonistic one. That is, conscientious outdoor users are discursively encouraged to see themselves as dangerous invaders in wilderness. Thus, implementation of the minimum impact protocols becomes a requirement to save wilderness. LNT standards are not optional when one sees oneself as inherently damaging to natural nature. A will to engage, on the other hand, allows for other types of relationships with natural nature. These types of relationships could be ones of love and respect, but they would not necessarily require the rigid application of LNT rules.

---

[93] Cole (1997), 3.

The will to engage, as process, is not without its own risks, of course. The realisation that nature changes chaotically and/or unpredictably could also open the door for a lessening of minimum impact standards or even an increase in resource extraction. For this reason, I am urging more, rather than less, discussion over changes in wilderness areas, and I am hoping for an increase in clarity regarding the consequences of the actions and decisions of outdoor recreation users. Humans are partners in the collective, not masters. Respect for other members should be kept in the fore as we discuss how to engage with our environment. Chaos models and hierarchy theory remove the normative prescriptive basis that underlies management or minimum impact practices. That is, when natural nature is viewed as stable, management and minimum impact policies that do not alter nature in any way are encouraged while change becomes synonymous with disruption and is seen as unhealthy for the environment. If nature is chaotic or is so at one level but not another, this normative grounding is lost: chaotic or unpredictable change becomes another natural process. Regrettably, this same argument can be applied to increased industrial use of wilderness areas. Mining operations, for instance, could be justified as simply part of the chaotic changes that natural areas are undergoing anyway. The danger of authorizing more resource extraction increases the need for more discussion and involvement between the relevant actants (humans and nonhumans).

---

[94] See McAvoy (1987) who stated that one of the goals of OR is "[t]o develop an awareness of and appreciation for the natural environment" (463).

The more effectively nonhuman artefacts can be articulated with human ones in a propositional configuration, the more powerful that proposition becomes. This is true whether the collaborators are companies bent on extractive practices or researchers and wilderness users intent on fostering a different type of relationship with the natural world. As a result, I am not suggesting that adopting a will to engage will stop all threats to wilderness. My comments are aimed at showing how certain propositional configurations come to the fore while others are relegated more to the sidelines. Understanding the process by which certain configurations become more powerful enables scholars to engage in the process of convoking the collective and this increased engagement does not often happen under the current perspective. For example, some scholars have recognized that certain minimum impact regulations do not actually have the effect they claim.[95] Even so, there has been remarkably little attention given to this realisation. Minimum impact policies and standards are still promoted in a nearly uniform manner. Adopting a will to engage and being curious about how it is that low-impact practices have been encouraged/required for years, even in the face of evidence that some of them do not provide the benefits they claim, would direct researchers' attention to the process by which knowledge about natural nature is created. In the absence of this awareness, it is difficult to see how to revise the process of creating knowledge in OR so as to allow nonhuman actants a more significant role in the production of that knowledge. So, while I agree that the will to engage does not solve problems as much as suggest an alternative approach to dealing with threats, I also realise that continuing to see natural nature as 'out there,' independent of humans, and without its own type of agency has not stopped the threats to wilderness areas either.

Nevertheless, I believe that outdoor recreation, as a field, needs to recognize and engage with the paradoxical nature of wilderness. The challenge becomes not simply keeping all human change out of the natural world, but of asking ourselves what kinds of changes we would like to see. Humans have an impact on nature when they are in wilderness. There is no way to remove all impacts, and the quest to do so precludes the ability to celebrate some impacts. Some wilderness impacts may indeed be necessary evils (for example, hardening campsites), but there may also be some that are beneficial (for example, trails can actually increase biodiversity as plants are able to colonize new areas as a result of the increased penetration of sunlight that trails provide). The point is, we will not know whether or not there are positive changes or how to encourage them unless our perspective shifts such that we can investigate the possibility opened up by the realisation that impacts occur regardless of how well minimum impact practices are implemented. No matter what, when humans use wilderness areas, they leave traces, even when all the minimum impact policies are adhered to strictly. As Marsh suggested in 1970, "[t]o maintain the quality of the wilderness and wilderness experience new management techniques will be required."[96] One option would incorporate current nonlinear models in ecology, a deeper understanding of and appreciation for paradoxes, poststructural theory, and science studies into management techniques.

---

[95] See Bell (1997), Christensen and Cole (2000), Cole (1997), Hammitt and Cole (1987), McGivney (1998 and 2003).

---

[96] Marsh (1970), 132.

CHAPTER 8: CONCLUSION

Those who attack the idea of progress always seem to be certain of their own progressiveness. ~ Hans Kellner[1]

This dissertation applied Foucault's archaeology and genealogy to ecological and outdoor recreation discourses. The purpose in using these methodologies was first to underscore the discursive structure of ecological thought and second to connect the various lines of descent of this structure into issues and concerns in outdoor recreation discourse. Understanding the content and functioning of outdoor recreation discourse is enhanced to some degree by such an analysis; however, as it turned out, the relations between ecology and OR are not as robust or ubiquitous as was expected. As a result, it proved to be insufficient to simply trace, via a genealogy, the lines of descent from OR to ecology and the development of scientific rationality. Therefore, Chapter Three also included an analysis of the discursive structure (in other words, an archaeological analysis) of OR that highlighted the ways in which knowledge is produced and positioned in outdoor recreation independently from ecology.

A parallel path of analysis connected both ecology and OR to the larger framework of modernity and its associated power/knowledge regime. Both ecology and OR are embedded within this framework and, therefore, both also suffer from similar limitations and problems. Because of this shared context, OR cannot use ecology as a model for a poststructural position that recognises the importance and power of language. Language does matter; however, neither ecology nor OR have directed much attention toward the effects language has on human/nonhuman interactions. In the end, my two paths enabled an analysis that examined the influence ecology has had on OR and looked at ecology and OR as individual discourses situated within a larger modern context.

---

[1] Kellner (1998), 40.

Furthermore, my deployment of archaeology and genealogy shows how they can usefully be combined such that the insights from one complement those of the other. Thus, I first discussed the archaeology of ecology, and then I used this as a basis for examining power relations in OR discourse. Both ecology and OR can enhance their understanding of the interactions between humans and nonhumans by comprehending the ways in which they each produce and enunciate concepts, theories, and other discursive objects. Thus, I pointed to the manner in which ecology and OR attempted to divide and reject humans from nature. In OR, this practice of division and rejection is connected to the promotion of minimum impact policies that have material effects in the world. For example, the potential for surveillance of backcountry users is one effect of ensuring compliance with minimum impact protocols. The value of alterations to or development in wilderness areas is also assessed in terms of this modern division of humans from nature. In OR discourse, wilderness is produced as an authentic and pristine place, and this production has physical and material consequences in terms of the changes to wilderness areas that OR supports.

The application of genealogy to OR discourse led to a reformulation of the agency of nonhumans. Genealogy traditionally conceptualizes agency in terms of human actors. When one considers discourses that deal with nonhumans, the opportunity to expand how poststructural theory conceives agency becomes evident. That is, I discussed how poststructural theory understands who agents are and how agency works. Then, I critiqued this understanding of agency in terms of its silences. For example, who cannot be agents according to poststructural theory? What areas does poststructural discourse remain silent about? Can poststructural theory justify those silences?

Challenging poststructural theory in terms of its conceptualization of agency led me to incorporate a science studies perspective that specifically theorizes the role nonhumans play in crafting our common world or collective.[2] According to science studies, nonhumans are actants in the process of convoking our collective. Thus, where poststructural theory fell short, science studies provided an alternative that showed that nonhumans are actually more involved in the functioning of both ecological and outdoor recreation discourse. In fact, using the concepts of the trace, différance, spectres, we could see how nonhumans, in the form of natural nature, return, albeit in an unacknowledged fashion, to affect both these discourses.

I applied a poststructural theory and a science studies perspective to the history of ecology from 1913 to 2000 and to outdoor recreation discourse, specifically those portions of it that deal with minimum impact and management of wilderness areas, from the 1960s onward. All discourses, Foucault suggested, perform internal divisions that separate some objects from others. The way in which any one discourse does this dividing up/out is unique. This is, in fact, a key component in the definition of discourse: discourses are unique series of manoeuvres that produce a particular configuration of arrangements and relationships between the objects, concepts, statements, and other elements they produce. Foucault identified a number of salient rules that govern discourse. For instance, not everything that it is possible to say is actually said in any one discourse; therefore, Foucault argued, discourses divide utterances into acceptable and unacceptable ones. By looking at ecological discourse in terms of how it constructs agency, we saw that some agents are accorded a more powerful position in the discourse when compared with other types of agents. Thus, in addition to division and rejection, statements and concepts are also subjected to various prohibitions (rituals and the right to speak, for example) that invest certain subject positions and concepts with more legitimacy. This is depicted graphically below (Figure 8.1).

---

[2] The idea of a collective as opposed to a society was discussed in Chapter Seven. Briefly, it refers to the collection of entities that make up our common world. These include human culture, certainly, but also nonhuman actants. The idea of a collective avoids the problem of culture versus nature that plagues so much research in ecology and OR.

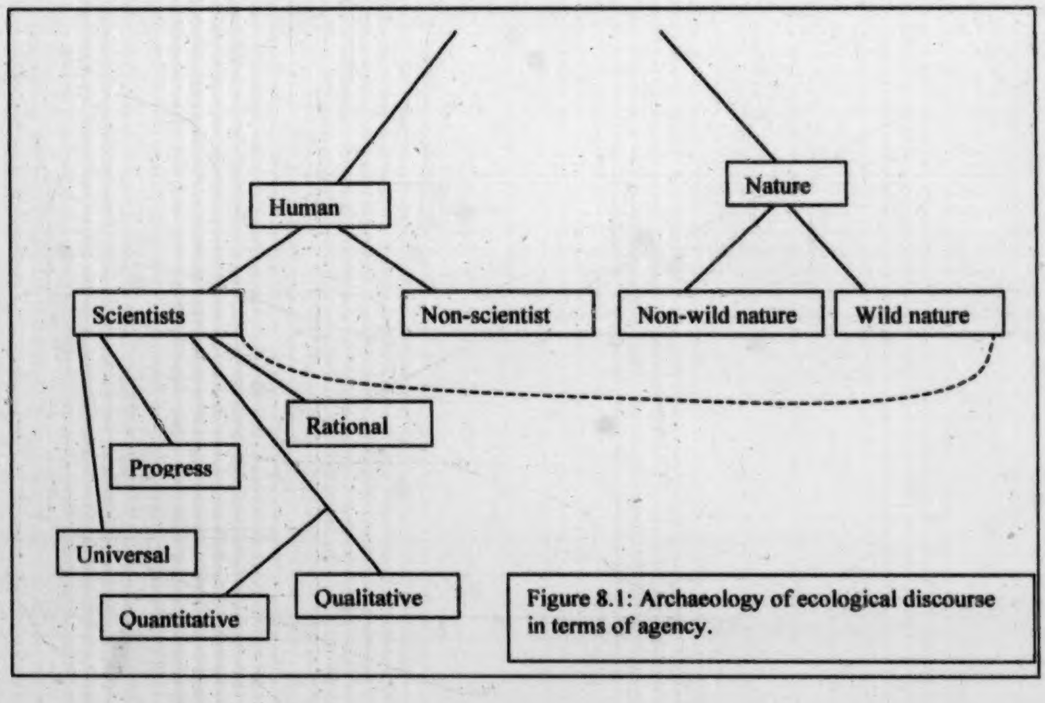

Figure 8.1: Archaeology of ecological discourse in terms of agency.

In ecological discourse, humans are divided from nature and are accorded the status of agent while nature remains the passive recipient of the actions and intentions of people. Humans are subsequently divided into scientists and non-scientists. The knowledge that scientists produce is also subjected to a number of divisions that have a privileged side. For example, knowledge that is quantitative is more valued than qualitative knowledge. Likewise, progressive and developmental knowledge occupied a privileged position. The rules that govern this particular pattern of division in ecology are the same as those of science in general (for example, 'scientist' is composed of similar aspects in chemistry as were seen in ecology).

In addition to analysing ecological discourse from the perspective of agency, I also focussed on the nonhuman axis. Doing so produces the following picture (Figure 8.2).

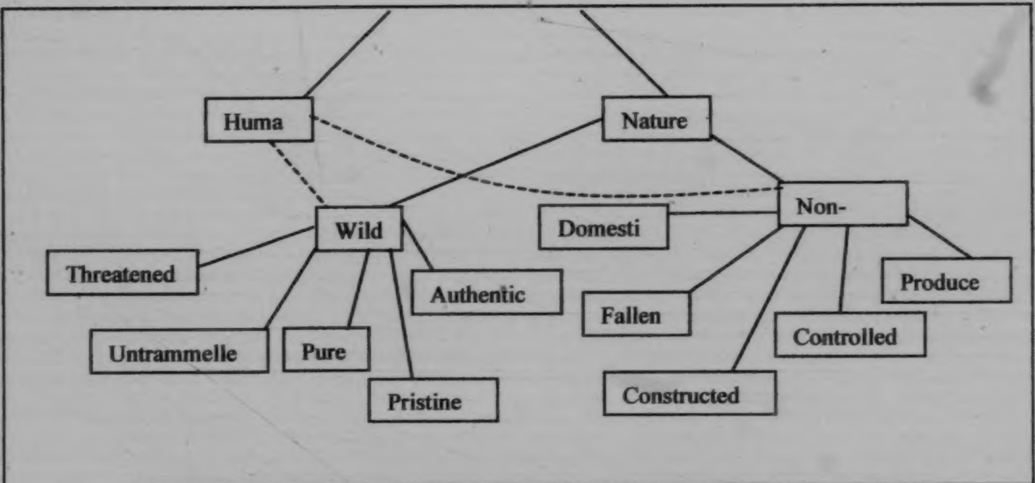

Figure 8.2: Archaeology of ecological discourse in terms of the nonhuman axis. This is particular to that part of ecological discourse that does not deal with human systems such as agriculture.

A focus on the nature side of the divide shows that ecological discourse separates various aspects of nature into wild and non-wild, which are then subsequently divided into other categories. Each of these divisions is accompanied by rituals, doctrinal rules and privileged rights to speak. Wild nature, for example, when spoken about by scientists, through rituals associated with the scientific method and subject to disciplinary norms, that are themselves associated with particular enunciative modalities (for example, research centres and universities) becomes articulated more authoritatively in the discourse.

The doted lines in each of these schematics represent an invisible relationship that often appears in the discourse as a silence or gap. That is, ecological discourse has rejected human culture to the degree that it is difficult to see the relationship between humans and wild nature. When scientists engage the discursive structure of ecology, they have already tacitly agreed to abide by the rules of formation. Therefore, they begin with the division of humans from nature. This separation becomes a condition for research that is accepted prior to the commencement of the research. The desirability of such a separation is rarely investigated and, thus, potential connections between ecologists and the nature they study are hidden by these discursive requirements. As a result, ecological discourse has been largely silent about the role scientists have in producing wild nature.

This silence is ~~one~~ of the results of the rules of formation in ecological discourse: humans become less connected, if not unconnected, to natural nature. This silence, it must be noted, does not indicate an absence of effect. That is, natural nature and humans are connected in a relationship whether or not the internal organization of ecological discourse admits it. The effects of this invisible relationship often appear as a surprise to the scientist. For instance, when Hayward tried to locate the chaparral but could not, he expressed a certain amount of surprise and frustration.[3] Ecological discourse does contain mechanisms for absorbing this surprise: Hayward attempted to fit his understandings of the chaparral into an over-arching developmental schema. I argued that in terms of the progressiveness of truth claims, the modern power/knowledge regime has a puzzling way of stitching together contradictions, errors, and failures into a seamless narrative. In other words, Hayward saw previous work as incomplete, inaccurate, or vague when compared to his own. As a result, ecologists were able to participate in the basic structure of the discourse, while at the same time addressing the confusing tendency for nature to appear in various and conflicting ways.

---

[3] Hayward (1948). This example was discussed in Chapter Five.

The structure of outdoor recreation discourse did prove to be linked in some ways with ecological discourse. OR discourse, for instance, makes a similar division and rejection of humans from natural nature as ecological discourse. Similar rituals (for example, the use of quadrats), an emphasis on quantitative data, and the privileging of particular subject positions from which scholars could speak more authoritatively about nature are indicative of some of the shared discursive practices between ecology and OR. Furthermore, both ecology and OR are products of modernity and, thus, operate within a will to know that emphasizes truth over fiction. The truth about natural nature is approached, according to the rules of formation, through a perceived refinement and development of knowledge over time. That is, modern science requires and tries to produce a distanced, uninvolved observer who makes sense out of what he or she is studying by coming to a more accurate, penetrating, and revealing explanation.

Outdoor recreation discourse is also distinct from ecological discourse in some ways. Its (stated) goal is to protect/save wilderness. Toward this end, the OR field supports a number of strategies and tactics whose purpose is to improve the quality and quantity of wilderness. These strategies and tactics include increasing the potential for surveillance, creating standards for both knowledge and behaviour, normalizing those standards (in some cases by vilifying alternative behaviours and/or forms of knowledge), creating and privileging certain subject positions (for example, wardens, guides, LNT instructors, researchers/scientists, and managers), and building certain processes, institutions, programs, and equipment that help to protect wilderness. OR also had another goal: protect the wilderness experience from eroding, which helps explain the shifts from detailed investigations of the ecology of an area to the experiences those areas could or should provide for users. This shifty/ing language is one manifestation of the silences or gaps that accompany all discourse. Outdoor recreation discourse remains silent about the shifty/ing language even as it performs this shift.

The silences or invisible relationships between humans and nature offer a point of entry for poststructural analysis. Even though these relationships are discursively visible as silences, they are still at work affecting the discourse. They have effects that can be seen. The poststructural concepts of the trace, différance, and hauntology offered insight into the significance of these effects. I read the paradox of managing natural nature so as to produce it as un-produced as an example of the haunting of nature by humans. Likewise, the use of scare quotes in ecology and OR for key concepts such as *natural*, *pristine*, and *authentic* can be understood as an attempt by authors to recognize the complexity of the topic while remaining within a discursive structure that refuses to admit the ambiguity of these concepts.

In the end, paradoxes, shifty/ing language, and scare quotes provide direction for an alternative configuration of the rules of formation in ecology and OR. I continued the analysis in a different vein and offered some thoughts on an alternative discursive formation for OR (and to a lesser extent in ecology). Consequently, I turned to science studies and develop a theory of paradoxes. I combined the insights from science studies and poststructural concepts with an understanding of paradoxes to suggest that ecology and OR could take language effects seriously and recognise that, in addition to human beings, other actants are involved in crafting our mutual world. I suggested a move away from the assumed neutral view of science to one that expressly engages with nonhumans as co-producers of knowledge in ecology and OR. Science studies offers a detailed process for how it is that agency can be distributed across various actors, including nonhumans. I found that the quantity of paradoxes present in ecology and OR, which at first seemed distressing, turned out to be a fruitful direction of analysis. In many cases, ecology and OR treat paradoxes negatively or flippantly. This treatment devalues paradoxes.

Researchers in both disciplines have commented on the presence of paradoxes. In ecology, even in the newer models that expressly concern themselves with chaos, randomness, and stochasticity, paradoxes are not usually seen as fruitful opportunities to engage in new discursive practices. In modernity, the will to know positions knowledge as inherently progressive and, thus, relegates paradoxes to the sideline. In ecology, the problems with some of its basic tenets (such as natural nature) have historically been viewed as problems of definition, methodology, and/or analysis. In OR, the response to paradoxes has been quite similar: researchers have tended to re-interpret them, reduce them to simple binary choices, or view them largely as curiosities worth mentioning in passing only; rarely do scholars consider paradoxes as fecund sources of insights. This view of paradoxes does not mean that ecology and OR do not recognize the complexity of their subject matter. Over time, theories and models in both disciplines have become more sophisticated and robust. Nevertheless, the approach to paradoxes that OR and ecology have taken has not paralleled this increase in complexity and sophistication. In many ways, paradoxes are treated in accordance with the modernist conceptualization of science as a progressive and developmental discipline.

What I propose is actually a shift in the rules that govern the production of knowledge in the discourses of ecology and OR. As mentioned in Chapter Two, the rules governing ecology changed little over the twentieth century, even as the objects it produced changed fairly dramatically. However, we can see in the last few decades (since about 1975) hints that these rules could change. In ecology, newer models are being developed that more clearly acknowledge and even embrace uncertainty, chaos, and stochasticity. These concepts offer an interesting challenge to the basic rules of science. Knowledge that is uncertain has most often been viewed in science as less valid or reliable. Now, ecology is showing that uncertainty is not necessarily a shortcoming. However, newer models in ecology do not combine readily with a will to know that seeks universality, standardization, and predictability. Outdoor recreation discourse, like ecology, has been largely governed by this will to know and, consequently, sanctions against other forms of knowledge that do not coincide with modern conceptualizations of truth and certainty.

Instead of a will to know, I suggest what might be called a will to engage. Were ecology and OR to be guided by such a structure they would benefit in five ways. First, the paradoxes they encounter would be more fully engaged. Currently, paradoxes threaten the very rules of formation and run counter to the techniques of the modern power/knowledge nexus. The processes by which paradoxes are produced as objects of knowledge cannot be contained within the discourses of ecology and OR. For this reason, neither ecology nor OR can see them clearly or deal with them effectively. In OR, especially, the tension that exists between the desire to connect people to nature and the premise that people are inherently destructive to nature receives little attention. An effective place to begin to address this tension, I contend, is the rules of formation that produced human beings as antithetical to nature. However, OR does not engage the problem at this level because language is thought to be a benign vehicle for expression. OR has remained silent concerning the power language has to co-produce our reality. Thus, the role the larger discursive surface of modernity and science has had in producing the paradoxes that pepper the field has not been investigated.

Second, ecology and OR would remain open to more revision because they would be able to engage with their own responses to questions about the world. Neither ecology nor OR find the Truth; they make the truth; they design it. By following a modern view of Truth, neither ecology nor OR stand accountable for the truth they find: the Truth awaits discovery out in the world. Thus, ecology and OR take the fact that human beings are antithetical to nature as independent of the work each discipline has done to produce that fact. However, this fact, along with every fact, is a product of the interaction of discourses with the collective world. The revision I am pushing ecology and OR to be open to is more than just a change in the conclusions they reach. Instead, I am arguing that ecologists and OR scholars ask themselves if the facts they help build are the ones they want to build. If our discourses help build facts, then the potential arises for us to build alternative ones. New questions appear. For example, we might ask whether or not we are satisfied with the orientation that human beings are antithetical to nature. Furthermore, we can begin the process of building new propositional configurations that re-structure the relationship humans have with nonhumans.

Third, and related to the second benefit, the types of actants involved increase when one engages with the process of creating the world. That is, ecology and OR can focus more on how nonhumans are implicated in the way we make sense out of the world. I do not subscribe to the view that we can simply turn off our cultural embeddedness and listen to nature in its own voice. I do believe, however, that we can listen more closely to nonhuman actants as they propose themselves as matters of concern to be studied. In fact, listening in this way begins by attuning ourselves to the surprises science often triggers. When we listen to these actants, we engage more, not less, with the collective. We cease to be the self-contained, autonomous scientist or researcher and become one more element in the process of making the world real, a process in which we are not the only actants.

Fourth, a will to engage also offers a broader spectrum of tools and techniques for understanding our collective. Most scientists and OR planners and researchers use rational, logical decision-making to comprehend the natural world. Logic, however, has some limitations, especially when it comes to chaos, paradoxes, and hauntings, which contain logic and at the same time go beyond it. A will to engage encourages us to employ a wider scope of tools and techniques in coming to decisions about how our collective is produced and in supporting various actions and behaviours in that collective. Furthermore, a will to engage reminds scholars to search beyond their own disciplinary boundaries. In doing so, OR researchers would have the opportunity to engage newer ecological theory that challenges the concept of a balanced wilderness to which most OR scholars subscribe.

Finally, adopting a will to engage would reduce the contempt for humanity that much of ecology and OR exhibit. This contempt is clear in OR studies that argue for the constant removal of all signs of human presence in the wilderness. On the one hand, OR strives to increase backcountry use, but, on the other hand it fears that very use will destroy the wilderness. Imperative commands, such as 'Stay on the trail!' and 'Leave No Trace!,' riddle OR discourse and speak to the level of contempt this field has for humans. These types of imperatives suggest that people will not act responsibly and, thus, need to be ordered to comply.

A couple of options have been explored over the past decade or so that may prove helpful in coming to a deeper understanding of actants, agency, and power relations in OR: adaptive and ecosystem management. Adaptive management strategies help diverse stakeholders to come together with a common vision and "provide the best prospect for conserving natural areas" because they allow for "novel ways of adapting to the persistent tendencies of ecosystems to surprise—to change in ways that we do not expect."[4] Adaptive management explicitly values a diversity of perspectives and stakeholders and could allow for a consideration of various types of actants, including nonhumans. At present, adaptive management plans do not consider nonhumans as participants; however, the commitment to including diverse stakeholders could be expanded to encompass nonhumans. Furthermore, as Patterson, Niccolucci, and Marchettini note, "[h]umans often fail to build self-organizing or adaptive capacities into their technologies and economic designs."[5] Because adaptive management "treats management policies as experiments that probe the responses of the system as human behavior changes," the possibility exists of building self-organizing capacities that include human discursive systems.[6] The role of discourse in adaptive management is highlighted by Bryan G. Norton, who argued that "the failures of environmental policy are linked to failures of language and communication—specifically communication between scientific disciplines and between the domains of science, values, and governance."[7] To this list, I would add the failure to communicate with those artefacts that are part of the process of real-*ising* the collective.

---

[4] Bocking (2000), xvii.
[5] Patterson, Niccolucci, and Marchettini (2008), 414.

In keeping with these suggestions, Lister and Kay maintain that adaptive management should follow three criteria: (1) more voices, values, perspectives, and forms of knowledge must be used, (2) collaborative processes need to be developed, and (3) more innovative and diverse planning tools should be adopted.[8] Underlying these criteria is an acceptance of paradoxes: "[a] new era is possible in which we value and celebrate the diversity of life, that while it confounds and frustrates, also inspires and motivates the human spirit. This is a paradox of life to be embraced by the challenge of *doing* adaptive planning, and therefore, designing *within* nature."[9] Adaptive management planning, as Lister and Kay explain it, recognizes chaos in nature, unpredictability, competing interests and values, and multiple correct responses. If OR were to take a similar approach when designing minimum impact policies and practices or discussing the goal(s) for wilderness areas, more voices, perspectives, values, and forms of knowledge would be consulted in a collaborative process.

---

[6] Patterson, Niccolucci, and Marchettini (2008), 408.
[7] In Howarth (2007), 456.
[8] Lister and Kay (2000).
[9] Lister and Kay (2000), 212, emphases in original.

At a fundamental level, adaptive management strategies offer the possibility that humans and nonhumans could exist harmoniously, even in the wilderness. By including nonhumans in the discussion, we can switch from an adversarial position to a co-operative one where both humans and nonhumans have a place at the table. Of course, adaptive management requires that humans adapt. If nonhumans are an integral part of the discussion, then human users of wilderness areas must be responsive and responsible to nonhuman needs. However, I believe that OR currently constructs nonhumans as needing the complete removal of human beings from wilderness. This places OR in an impossible situation: wilderness areas cannot be used when people must be completely removed from these areas. The dismal ecological state of many of Canada's protected areas strongly suggests that our current approach is not working. Nonhuman actants are clearly articulating their need; yet, for two reasons we do not hear them. First, modern science does not allow nonhumans a voice, and, second, the antagonistic relationship humans are thought to have with wilderness pre-emptively constructs nonhumans as needing the complete absence of humans in order to function.

Ecosystem management is another approach that has the potential for incorporating some of my suggestions. Ecosystem management combines natural and cultural perspectives and is therefore more holistic than models that base decisions solely on scientific data about nature. Parks Canada has defined ecosystem management as "the integrated management of natural landscapes, ecological processes, physical and biotic components, and human activities, to maintain or enhance the integrity of an ecosystem."[10] Along with adaptive management, ecosystem management makes room for both cultural and natural factors. While I have argued that the separation of culture from nature is neither feasible nor desirable, ecosystem management does enable a wider consideration of the factors and processes involved and, thus, could include the kinds of actants I have suggested are an important element. Finally, both adaptive management and ecosystem management have the potential to keep decisions open even as action is taken, because they explicitly recognise the impossibility of obtaining absolutely accurate and complete information. Ecosystems display a persistent habit of changing in ways that we cannot predict; thus, our decisions about ecosystems, regardless of how carefully we make then, cannot remain appropriate indefinitely.

---

[10] Quoted in Swinnerton (1999), 206.

Both adaptive and ecosystem management emphasize the inclusion of a variety of human stakeholders when decisions are made regarding the management of natural areas. This inclusion is in keeping with my suggestions concerning the incorporation of human thinking into ecology and OR discourse. At issue here is the scope of the problem: are human beings outside the problem, looking in and applying a scientific lens, or does the problem encompass humans and their modelling practices? What is the frame of reference? Adaptive and ecosystem management hint at a larger scope and I would increase it still more. Hence, an independent nature can be created by the power exercised through ecology's gaze. This independent nature, in turn, exercises power to produce its own effects on the human subjects/objects that invented the independence of nature. Taking this further, the supersystem collective has actually been part of the process that produced a human consciousness that has in turn produced an ecological science that is studying larger and more complex systems until now it has arrived at the point where that science can no longer count itself outside the system. Human consciousness, thus, is not simply autonomous; it is also part of the ecology of the world. And this ecology has produced the situation where poststructural and scientific thinking turned on themselves in a productive manner and began to deconstruct deconstruction. This paradoxical ecology includes itself in the world of constructed, but independent, objects of knowledge. This situation now moves us beyond some very fundamental binaries and makes it difficult to speak unproblematically about subjects and objects, cause and effect, and relativism and absolutism. We now have to engage a situation whereby neither pole of these binaries exists. We have no truth, but equally, we have no fiction. We are not autonomous, self-conscious beings looking at the world, but neither is the world an internal state produced solely by human perception. It would be exciting to see how/whether adaptive or ecosystem management is amenable to such an argument.

Adaptive and ecosystem management are two relatively new approaches to maintaining the integrity of wilderness areas over time. My comments regarding them are based on initial similarities and commonalities. The potential to integrate them into OR discourse seems, at this stage, to be promising, although there will be challenges and limitations. For example, the ideas of power-knowledge and the disciplinary regime that I discussed in Chapters Two and Three and the poststructural concepts of Chapter Five were helpful in seeing the effects of discursive rules in ecology and OR, but it is not immediately clear how this kind of analysis could be included in adaptive or ecological management plans. I suspect that there is potential here for a productive interaction between adaptive or ecological management and the type of paradoxical ecology and OR that I have put forth; however, that potential has yet to be realised.

The will to engage is a perpetual process without end. Accordingly, discussions would not end when application of particular management policies or minimum impact protocols begins. In a sense, then, discussions of conceptual and theoretical concerns are now part of doing science, which, in turn, is part of management policy development and minimum impact research agendas. The will to engage also indicates that science can never explore, or even be aware of, all the implications of the knowledge it creates, because as elements within propositional configurations are added, subtracted, and substituted different implications are produced. When we think we understand all the implications, we close off the will to engage. As a result, there are implications of adopting a will to engage, which I have not explored, that will have to await further discussion.

One unexplored implication of the will to engage concerns the role that rules of formation play in giving bodies of knowledge certain patterns and propensities. For instance, we might understand that both ecology and OR produce knowledge that privileges certain subject positions, orientations toward the environment, political agendas, and economic structures (to identify only some of the factors involved). If some of the environmental and social problems can be tied to the rules of formation that ecology and OR employ, a more comprehensive understanding of the rules of formation may help reduce the problems. However, a deeper understanding of the details awaits further discussion.

In addition to the suggestions for ecology and outdoor recreation discourse, this dissertation also contributes to the development of environmental history as a discipline. Environmental historians have written about ecology and conservation. These scholars have traditionally chronicled the changes and developments in what they determined was the scope of their study. So, for example, scholars have written on the history of ecological concepts, the historical use of metaphors in ecology, the relationships between ecologists and government over certain periods of time, the connections between ecological science and influential social forces of the day (for instance, the Cold War's influences on ecology). One area that has not received much attention until now is the relationship between ecology and outdoor recreation. Given the amount of backcountry use in North American, the challenges of managing and protecting wilderness lands, and the recent developments in ecological theory, it is fair to say that the relationship between ecology and OR is an important point to consider. Unless we understand how ecological principles are applied outside of that discipline, we will not fully understand the importance and function that ecology plays in our world. I offer one example of how ecological principles are (or are not) engaged outside the discipline.

I would like to end with a final word about progress and paradoxes. Paradoxes are both logical and illogical. A paradoxical process for generating knowledge in ecology or outdoor recreation is, thus, partly illogical. Making suggestions or arguments for what should be done in a particular situation is complicated by the presence of paradoxes. When the topic concerns the integrity and survival of wilderness area, scholars face the challenge of making suggestions while at the same time remaining open to paradoxes. Certainly, I think the process outlined here has something to recommend. Nonetheless, I realise that in suggesting its adoption, or at least consideration, I am also positioning it as an improvement on older models and processes. It is problematical to criticise OR discourse, and the concept of the accumulation of progressively developmental knowledge, with the aim of offering an improvement. I do not wish my suggestions to be taken as absolute truth claims, but neither do I see them as having no value. The space in between the scepticism of progress and the desire to contribute to the protection and integrity of wilderness marks the location where I entered into the discussion.

The opening quotation in this chapter, from historian Hans Kellner, states an interesting conundrum that gives me pause to think: how can I critique the idea of progress? When I am confident in the progressiveness of such a critique, I undo that which I have wrought. The more effective the critique of progress, the more forceful the conundrum. Thus, I do not claim that my position takes a step closer to Truth; nor do I claim that my position is superior (in a progressively developmental sense) to current discursive practices in ecology or OR. Certainly, I do not wish to invalidate myself; however, I would like to end with this paradox: the more convincingly I have made my argument, the better it can be used against itself. And that is just fine with me.

Reference List

Abbott, Katrina. September 1995. Outward Bound and wilderness. *International Journal of Wilderness* 1, no. 1: 23-26.

Adamson, R. S. June 1918. On the relationships of some associations of the southern Pennines. *The Journal of Ecology* 6, no. 2: 97-109.

Allan, H. H. February 1926. Epharmonic responses in certain New Zealand species, and its bearing on taxonomic questions. *The Journal of Ecology* 14, no. 1: 72-91.

Altmeyer, George. 1995. Three ideas of nature in Canada, 1893-1914. In *Consuming Canada: Readings in environmental history*, ed. by Chad Gaffield and Gaffield Pam, 96-118. Toronto, Ontario: Copp Clark.

Armitage, Eleonora. 1918. On the habitats and frequencies of some Madeira Bryophytes. *The Journal of Ecology* 6, no. 3: 220-225.

Asdal, Kristin. 2003. The problematic nature of nature: The post-constructivist challenge to environmental history. *History and Theory Studies in the Philosophy of History: Environment and History* 42, no. 4: 60-74.

Attarian, A. October 1996. Integrating values clarification into outdoor adventure programs and activities. *Journal of Physical Education, Recreation and Dance* 67, no. 8: 41-44.

Attarian, Aram. 2001. Trends in outdoor adventure education. *Journal of Experiential Education* 24, no. 3: 141-149.

Bachert, Delmar W. 1990. Historical evolution of NOLS: The National Outdoor Leadership School. In *Adventure education*, ed. John C. Miles and Simon Priest, 83-88. State College, PA: Venture Publishing.

Bakker, R. Scott. 2005. *The Thousandfold Thought: The Prince of Nothing Book Three*. Toronto: Penguin Group.

Bard, Gily E. July 1952. Secondary succession on the piedmont of New Jersey. *Ecological Monographs* 22, no. 3: 195-215.

Barton, Andrew M. 1993. Factors controlling plant distributions: Drought, competition, and fire in montane pines in Arizona. *Ecological Monographs* 63, no. 4: 367-297.

Battin, J. G. and J. G. Nelson. 1982. Recreation and conservation: The struggle for balance in Point Pelee National Park, 1918-1978. In *Recreational land use: Perspectives on its evolution in Canada*, ed. Geoffrey Wall and John S. Marsh, 77-101. Ottawa: Carleton University Press.

Baydack, Richard K. 2000. Science and biodiversity. In *Biodiversity in Canada: Ecology, ideas, and action*, ed. by Stephen Bocking, 175-187. Peterborough, ON: Broadview Press.

Beckwith, Stephen L. October 1954. Ecological succession on abandoned farm lands and its relationship to wildlife management. *Ecological Monographs* 24, no. 4: 349-376.

Begon, Michael, John L. Harper, and Colin R. Townsend. 1990. *Ecology: Individuals, populations and communities*. 2nd ed. Boston: Blackwell Scientific Publications.

Bélanger, L. René Gagné, Guy Parent, Marius Pineau, and Jean-Claude Ruel. 1990. L'apport de l'inventaire ecologique à l'aménagement des espaces récréatifs en milieux naturels. *Loisir et Société* 13, no. 2: 325-357.

Bell, Anne C. 1997. Protecting the Tatshenshini: Wild nature as a resource? In *Canadian issues in environmental ethics*, ed. Alex Wellington, Allen Greenbaum, and Wesley Cragg, 219-228. Peterborough, Ontario: Broadview Press.

Belsey, Catherine. 2002. *Poststructuralism: A very short introduction*. Oxford: Oxford University Press.

Benedetti-Cecchi, Lisandro. 2000. Predicting direct and indirect interactions during succession in a mid-littoral rocky shore assemblage. *Ecological Monographs* 70, no. 1: 45-72.

Bennett, Tony. 1994. The exhibitionary complex. In *Culture/Power/History: A reader in contemporary social theory*, ed. Nicholas Dirks and Geoff Eley, 123-154. Princeton: Princeton University Press.

Berard, T. J. 1999. Michel Foucault, the history of sexuality, and the reformation of social theory. *Journal for the Theory of Social Behaviour* 29, no. 3: 203-227.

Berger, C. 1983. *Science, God, and nature in Victorian Canada*. Toronto, Ontario: University of Toronto Press.

Beringer, Almut. 2004. Toward an ecological paradigm in adventure programming. *Journal of Experiential Education* 27, no. 1: 51-66.

Bews, J. W. December 1916. An account of the chief types of vegetation in South Africa, with notes on the plant succession. *The Journal of Ecology* 4, no. 3/4: 129-159.

Biersack, Aletta and James B. Greenberg. 2006. *Reimagining political ecology*. Durham: Duke University Press.

Blangy, Sylvie and Todd Nielson. 1993. Ecotourism and minimum impact policy. *Annals of Tourism Research* 20, no. 2: 375-360.

Bocking, Stephen. 2000. Encountering biodiversity: Ecology, ideas, action. In *Biodiversity in Canada: Ecology, ideas, and action*, ed. Stephen Bocking, ix-xxv. Peterborough, ON: Broadview Press.

Borrie, William T. 2001. The dynamic, emergent, and multi-phasic nature of on-site wilderness experiences. *Journal of Leisure Research* 33, no. 2: 202.

Bowler, Peter J. and Iwan Rhys Morus. 2005. *Making modern science: A historical survey*. Chicago: University of Chicago Press.

Boyd, David R. 2003. *Unnatural Law: Rethinking Canadian environmental law and policy*. Vancouver: UBC Press.

Boyd, Stephen W. and Richard W. Butler. 1996. Managing ecotourism: An opportunity spectrum approach. *Tourism Management* 17, no. 8: 557-566.

Bracken, Christopher. 1997. *The potlatch papers: A colonial case history*. Chicago: University of Chicago Press.

Bramwell, Anna. 1989. *Ecology in the 20th century: A history*. London: Yale University Press.

Braun, E. Lucy. April 1947. Development of the deciduous forests in eastern North America. *Ecological Monographs* 17, no. 2: 211-219.

Breiturg, Denise L., Timothy Loher, Carol A. Pacey, Adam Gerstein. 1997. Varying effects of low dissolved oxygen on trophic interactions in an estuarine food web. *Ecological Monographs* 67, no. 4: 489-507.

Brenchley, Winifred E. and Helen Adam. December 1915. Recolonisation of cultivated land allowed to revert to natural conditions. *The Journal of Ecology* 3, no. 4: 193-210.

Brissette, Aria P., Glenn E. Haas, and Delwin E. Benson. Winter 2001. Justifications for recreational carrying capacity: What the public is willing to accept. *Journal of Park and Recreation Administration* 19, no. 4: 22-41.

Broch-Due, Vigdis. 2000. Producing nature and poverty in Africa: An introduction. In *Producing nature and poverty in Africa*, ed. Vigdis Broch-Due and Richard A. Schroeder, 9-52. Stockholm: Nordiska, Afrikainstitutet.

Broch-Due, Vigdis and Richard A. Schroeder. 2000. *Producing nature and poverty in Africa*. Stockholm: Nordiska, Afrikainstitutet.

Brown, Dalton Milford. January 1941. Vegetation of Roan Mountain: A phytosociological and successional study. *Ecological Monographs* 11, no. 1: 61-97.

Brown, Robert Craig. 1970. The doctrine of usefulness: Natural resource and national park policy in Canada, 1887-1914. In *Canadian parks in perspective*, ed. J. G. Nelson and R. C. Scace, 46-62. Montreal, Quebec: Harvest House.

Bunting, Camille J. 1990. Interdependency: A key in environmental and adventure education. In *Adventure education*, ed. John C. Miles and Simon Priest, 453-458. State College, PA: Venture Publishing.

Burdge, Rabel J. and Donald R. Field. 1972. Methodological perspectives for the study of outdoor recreation. *Journal of Leisure Research* 4, no. 1: 63-72.

Butler, Richard. 1989. The future. In *Outdoor recreation in Canada*, ed. Geoffrey Wall, 277-310. Toronto: John Wiley & Sons.

Cain, Kelly. 1999. The burden and privilege of educating for environmental awareness. *Journal of Experiential Education* 22, no. 3: 117, 122.

Cain, Stanley A. April 1947. Characteristics of natural areas and factors in their development. *Ecological Monographs* 17, no. 2: 185-200.

Camp, Richard J. and Richard L. Knight. 1998. Rock climbing and cliff bird communities at Joshua Tree National Park, California. *Wildlife Society Bulletin* 26, no. 4: 892-898.

Campbell, J. Michael. 2002/2003. Application of GIS to recreation hazard management: Strainers, public safety, and ecological integrity: Banff National Park. *Leisure/Loisir* 27, no. 1/2: 53-76.

Campbell, Jonathan E. and David J. Gibson. 2001. The effect of seeds of exotic species transported via horse dung on vegetation along trail corridors. *Plant Ecology* 157, no. 23-35.

Carter, Dick. 1997. Maintaining wildlife naturalness in wilderness. *International Journal of Wilderness* 3, no. 3: 17-21.

Carver, Steve, Andy Evans, and Steffen Fritz. 2002. Wilderness attribute mapping in the United Kingdom. *International Journal of Wilderness* 8, no. 1: 24-9.

Cavers, F. December 1914. Gola's osmotic theory of edaphism. *The Journal of Ecology* 2, no. 4: 209-231.

Chapin III, F. Stuart, Lawrence R. Walker, Christopher L. Fastie, Lewis C. Sharman. 1994. Mechanisms of primary succession following deglaciation at Glacier Bay, Alaska. *Ecological Monographs* 64, no. 2: 149-175.

Chesson, Peter L. 1981. Models for spatially distributed populations: The effect of within-patch variability. *Theoretical Population Biology* 19: 288-325.

Christensen, Harriet H. and Roger N. Clark. 1983. Increasing public involvement to reduce depreciative behavior in recreation settings. *Leisure Sciences* 5, no. 4: 359-379.

Christensen, Neal A. and David N. Cole. 2000. Leave No Trace practices: Behaviors and preferences of wilderness visitors regarding use of cookstoves and camping away from lakes. Paper presented at the wilderness science in a time of change conference—Volume 4: wilderness visitors, experience, and visitor management. Ogden, UT: Rocky Mountain Research Station.

Clark, James S. 1990. Fire and climate change during the last 750 yr in northwestern Minnesota. *Ecological Monographs* 60, no. 2: 135-159.

Clarke, George L. October 1946. Dynamics of production in a marine area. *Ecological Monographs* 16, no. 4: 321-337.

Clements, Frederic E. 1916. *Plant succession: An analysis of the development of vegetation*. Washington: Carnegie Institute of Washington.

Cohen, Michael P. January 1996. Comment: Resistance to wilderness. *Environmental History* 1, no. 1: 33-42.

Cole, D. N. 1995a. Disturbance of natural vegetation by camping: Experimental applications of low level stress. *Environmental Management* 19, no. 3: 405-416.

________. 1995b. Ecological manipulation in wilderness—an emerging management dilemma. *International Journal of Wilderness* 1, no. 1, http://ijw.wilderness.net/articles/ecologic.cfm (accessed March 3, 2008).

________. 1995c. Wilderness management principles: Science, logical thinking or personal opinion? *Trends* 32, no. 1: 6-9.

________. 1997. Experimental evaluations of two Leave-No-Trace techniques: Removing boots and using geotextile groundcloths (scrim). *Research Paper Intermountain Research Station, USDA Forest Service* INT-RP-497: 1-7.

________. August 2000. Natural, wild, uncrowded, or free? *International Journal of Wilderness* 6, no. 2: 5-8.

________. April 2002. Managing the density of recreation use in wilderness. *International Journal of Wilderness* 8, no. 1: 19, 45.

Cole, D. N., T. P. Hammond, and S. F. McCool. 1997. Information quantity and communication effectiveness: Low-impact messages on wilderness trailside bulletin boards. *Leisure Sciences* 19, no. 1: 59-72.

Colinvaux, Paul A. 1973. *Introduction to ecology*. New York: John Wiley & Sons.

Confer, John J., Andrew J. Mowen, Alan R. Graefe, and James D. Absher. 2000. Magazines as wilderness information sources: Assessing user's general wilderness knowledge and specific Leave No Trace knowledge. Paper presented at the Wilderness science in a time of change conference—Volume 4: wilderness visitors, experience, and visitor management Ogden, UT: Rocky Mountain Research Station.

Conley, Verena Andermatt. 1997. *Ecopolitics: The environment in poststructuralist thought*. London: Routledge.

Connolly, Sean R. and Joan Roughgarden. 1999. Theory of marine communities: Competition, predation, and recruitment-dependent interaction strength. *Ecological Monographs* 69, no. 3: 277-296.

Cook, Barbara and William Borrie. December 1995. Trends in recreation use and management of wilderness. *International Journal of Wilderness* 1, no. 2.

Cook Jr., Walter L. 1972. An evaluation of the aesthetic quality of forest trees. *Journal of Leisure Research* 4, no. 4: 293-302.

Cook, Robert Edward. 7 October 1977. Raymond Lindeman and the trophic-dynamnic concept in ecology. *Science* 198, no. 4312: 22-26.

Cooksey, Ray W., Terry L. Dickinson, and Ross J. Loomis. 1982. Preferences for recreational environments: Theoretical considerations and a comparison of models. *Leisure Science* 5, no. 1: 19-34.

Cooper, Charles F. April 1960. Changes in vegetation, structure, and growth of southwestern pine forests since white settlement. *Ecological Monographs* 30, no. 2: 129-164.

Cooper, William E. Autumn 1965. Dynamics and production of a natural population of a fresh-water amphipod, *Hyalella azteca*. *Ecological Monographs* 35, no. 4: 377-394.

Conrad, Margaret and Alvin Finkel. 2003. *Canada a national history*. Toronto: Longman.

Cordell, Ken H., Michael A. Tarrant, and Gary T. Green. August 2003. Is the public viewpoint of wilderness shifting? *International Journal of Wilderness* 9, no. 2: 27-32.

Cordell, Ken H., Carter J. Betz, and Gary T. Green. 2002. Recreation and the environment as cultural dimensions in contemporary American society. *Leisure Sciences* 24: 13-41.

Coupland, Robert T. October 1950. Ecology of mixed prairie in Canada. *Ecological Monographs* 20, no. 4: 271-315.

Cowan, Ian McTaggart. 1970. The role of ecology in the national parks. In *Canadian parks in perspective*, ed. J. G. Nelson and R. C. Scace, 321-328. Montreal, Quebec: Harvest House.

Cronon, William. 1995a. The trouble with wilderness; or getting back to the wrong nature. In *Uncommon ground: Toward reinventing nature*, ed. William Cronon, 69-90. New York: WW Norton & Company.

________. 1995b. *Uncommon ground: Toward reinventing nature*. New York: WW Norton & Company.

________. January 1996. The trouble with wilderness: A response. *Environmental History* 1, no. 1: 47-55.

Crompton, John L. 2002. The rest of the story. *Journal of Leisure Research* 34, no. 1: 93-102.

Dachnowski, Alfred. December 1914. The international phytogeographic excursion of 1913 and its significance to ecology in America. *The Journal of Ecology* 2, no. 4: 237-245.

Dambach, Charles A. July 1944. A ten-year ecological study of adjoining grazed and ungrazed woodlands in northeastern Ohio. *Ecological Monographs* 14, no. 3: 255-270.

Daniels, Melissa L. and Jeffrey L. Marion. 2004. Communicating Leave No Trace ethics and practices: Efficacy of two-day trainer courses. *Journal of Park and Recreation Administration* 23, no. 4: 1-19.

Daubenmire, R. October 1952. Forest vegetation of northern Idaho and adjacent Washington, and its bearing on concepts of vegetation classification. *Ecological Monographs* 22, no. 4: 301-330.

Davenport, Mae A. William T. Borrie, Wayne A. Freimund, Robert E. Manning. 2002. Assessing the relationship between desired experiences and support for management actions at Yellowstone National Park using multiple methods. *Journal of Park and Recreation Administration* 20, no. 3: 51-64.

Davis, Colin. 2005. Hauntology, spectres and phantoms. *French Studies* 59, no. 3: 373-379.

Dawson, Michael. Fall 1997. 'That nice red coat goes to my head like champagne': Gender, antimodernism and the Mountie image, 1880-1960. *Journal of Canadian Studies* 32, no. 3: 119-139.

D'Cruz, Carolyn. January 2006. Adjusting the tone of Marxism: A hauntological promise for ghosts of communism in a democracy-to-come. *Contretemps* 6: 59-73.

de Peralta, Fernando. July 1935. Some principles of competition as illustrated by sudan grass, *Holcus sorghum sudanensis* (Piper) Hitch. *Ecological Monographs* 5, no. 3: 355-404.

DeAngelis, D. L. and J. C. Waterhouse. March 1987. Equilibrium and nonequilibrium concepts in ecological models. *Ecological Monographs* 57, no. 1: 1-21.

Deluca, T. H., W. A. Patterson IV, W. A. Freimund, and D. N. Cole. 1998. Influence of llamas, horses, and hikers on soil erosion from established recreation trails in western Montana, USA. *Environmental Management* 22, no. 2: 255-262.

Dennis, Brian, Robert A. Desharais, J. M. Cushing, and R. F. Costantino. 1995. Nonlinear demographic dynamics: Mathematical models, statistical methods, and biological experiments. *Ecological Monographs* 65, no. 3: 261-281.

des Jardins, Joseph R. 1997. *Environmental ethics: An introduction to environmental philosophy*. 2nd ed. Belmont, CA: Wadsworth Publishing Company.

Derrida, Jacques. 1973. Différance. In *Speech and phenomena and other essays on Husserl's theory of signs*, trans. David B. Allison, 129-160. Evanston, Illinois: Northwestern University Press.

_______. 1978a. Cogito and the history of madness. In *Writing and difference*, trans. Alan Bass, 31-63. Chicago: University of Chicago Press.

_______. 1978b. Force and signification. In *Writing and difference*, trans. Alan Bass, 3-30. Chicago: University of Chicago Press.

_______. 1994. *Specters of Marx: The state of the debt, the work of mourning, and the new international*. Trans. Peggy Kamuf. New York: Routledge.

Dirks-Edmunds, Jane C. July 1947. A comparison of biotic communities of the cedar-hemlock and oak-hickory associations. *Ecological Monographs* 17, no. 2: 235-260.

Dreyfus, Hubert 2004. Heidegger and Foucault on the subject, agency and practices. http://ist-socrates.berkeley.edu/~hdreyfus/html/paper_heidandfoucault.html. Accessed March 14, 2008.

Driver, B. L. 1970. *Elements of outdoor recreation planning*. Ann Arbor: University of Michigan.

_______. 2002. Reality testing. *Journal of Leisure Research* 34, no. 1: 79-88.

Driver, B. L., Perry J. Brown, George H. Stankey, Timothy G. Gregoire. 1987. The ROS planning system: Evolution, basic concepts, and research needed. *Leisure Sciences* 9: 201-212.

Drury, Jack K. and Bruce F. Bonney. 1992. *The backcountry classroom: Lesson plans for teaching in the wilderness*. Saranac, New York: Wilderness Education Association.

Dufrene, Marc and Pierre Legendre. 1997. Species assemblages and indicator species: The need for a flexible asymmetrical approach. *Ecological Monographs* 67, no. 3: 345-366.

Duncan, Garrett S. and Steven R. Martin. August 2002. Comparing the effectiveness of interpretive and sanction messages for influencing wilderness visitors' intended behavior. *International Journal of Wilderness* 8, no. 2: 20-25.

Dunlap, Thomas R. January 1996. Comment: But what did you go out into the wilderness to see? *Environmental History* 1, no. 1: 43-46.

Dunlap, Riley E. and Robert. Bruce Heffernan. Spring 1975. Outdoor recreation and environmental concern: An empirical examination. *Rural Sociology* 40, no. 1: 18-30.

Dustin, Daniel, ed. 1985. *The management of human behavior in outdoor recreation settings*. San Diego: San Diego State University's Institute for Leisure Behavior.

________. 2002. One dog or another: Tugging at the strands of social science. *Journal of Leisure Research* 34, no. 1: 89-92.

Eagles, Paul F. J. 1985. Community values and the protection of environmentally sensitive areas. *Loisir et Société* 8, no. 1: 205-216.

Eberhardt, L. L. and J. M. Thomas. 1991. Designing environmental field studies. *Ecological Monographs* 61, no. 1: 53-73.

Ecological succession of mosses, Isle Royale, Lake Superior. September 1913. *The Journal of Ecology* 1, no. 3: 201-203.

Egerton, Frank N. 1977. *History of American ecology*. New York: Arno Press.

Egler, Frank E. July 1942. Vegetation as an object of study. *Philosophy of Science* 9, no. 3: 245-260.

Ehrlich, Paul R. and Jonathan Roughgarden. 1987. *The science of ecology*. New York: Macmillan Publishing Company.

Ellison, Lincoln. January 1957. Symposium on the applications of ecology: Applications of ecology—Concluding statement. *Ecology* 38, no. 1: 63-64.

Elton, Charles. October 1924. Periodic fluctuations in the number of animals: Their causes and effects. *British Journal of Experimental Biology* 2, no. 1: 119-163.

______. 1927. Animal Ecology. London: Sidgwick & Jackson.

Elton, Charles and Mary Nicholson. November 1942. The ten-year cycle in numbers of the lynx in Canada. *The Journal of Animal Ecology* 11, no. 2: 215-244.

Emerson, Alfred E. January 1939. Social coordination and the superorganism. *American Midland Naturalist* 21, no. 1: 182-209.

Ermath, Elizabeth Deeds. 2001. Agency in the discursive condition. *History and Theory Theme Issue* 40: 34-58.

Ewert, Alan W. 1995. Trends and issues in wilderness research: An introduction. *Trends* 32, no. 1: 2-5.

Ewert, Alan, Greg Place, and Jim Sibthorp. 2005. Early-life outdoor experiences and an individual's environmental attitudes. *Leisure Sciences* 27, no. 3: 225-239.

Ewert, Alan W. and Steven J. Hollenhorst. 1997. Adventure recreation and its implications for wilderness. *International Journal of Wilderness* 3, no. 2: 21-26.

Farrell, Tracy, Troy E. Hall, and Dave D. White. 2001. Wilderness camper's perception and evaluation of campsite impacts. *Journal of Leisure Research* 33, no. 3: 229-250.

Farrell, Terence M. 1991. Models and mechanisms of succession: An example from a rocky intertidal community. *Ecological Monographs* 61, no. 1: 95-113.

Faubion, James D. 1998. Introduction. In *Aesthetics, method, and epistemology*, ed. James D. Faubion. Vol. 2 of *Essential Works of Foucault, 1954-1984*, ed. Paul Rabinow, xiii-xliv. New York: The New Press.

Field, Donald R., ed. 1981. *Leisure Sciences* 4, no. 3. New York: Cran, Russak & Company.

Fisher, C. E., Jess L. Fults, and Henry Hopp. April 1946. Factors affecting action of oils and water-soluble chemicals in mesquite eradication. *Ecological Monographs* 16, no. 2: 109-126.

Flader, Susan L. 1994. *Thinking like a mountain: Aldo Leopold and the evolution of an ecological attitude toward deer, wolves, and forests*. Madison, Wisconsin: University of Wisconsin Press.

Forbes, Bruce C., James J. Ebersole, and Beate Strandberg. August 2001. Anthropogenic disturbance and patch dynamics in circumpolar arctic ecosystems. *Conservation Biology* 15, no. 4: 954-969.

Forman, Richard T. T. and Michel Godron. November 1981. Patches and structural components for a landscape ecology. *BioScience* 31, no. 10: 733-740.

Foster, Janet. 1978. *Working for wildlife: The beginning of preservation in Canada.* Toronto: University of Toronto Press.

Foucault, Michel. 1972a. *The archaeology of knowledge & the discourse on language.* Trans. A. M. Sheridan Smith. New York: Pantheon Books.

________. 1972b. The discourse on language. In *Archaeology of knowledge and the discourse on language*, trans. Rupert Swyer. 215-237. New York: Pantheon Books.

________. 1980. *Power/knowledge: Selected interviews and other writings, 1972-1977.* Edited by Colin Gordon, Leo Marshal, John Mepham, trans. Kate Soper. New York: Pantheon Books.

________. 1988. *Madness and civilization: A history of insanity in the age of reason.* Translated by Richard Howard. New York: Vintage Books.

________. 1995. *Discipline and punish: The birth of the prison.* Translated by Alan Sheridan, 2nd ed. New York: Vintage Books.

________. 1997a. The ethics of the concern for the self as a practice of freedom. In *Ethics: Subjectivity and truth*, ed. Paul Rabinow. Vol. 1 of *Essential Works of Foucault, 1954-1984*, ed. Paul Rabinow, 281-301. New York: The New Press.

________. 1997b. The will to knowledge. In *Ethics: Subjectivity and truth*, ed. Paul Rabinow. Vol. 1 of *Essential Works of Foucault, 1954-1984*, ed. Paul Rabinow, 11-16. New York: The New Press.

________. 1998a. Nietzsche, Freud, Marx. In *Aesthetics, method, and epistemology*, ed. James D. Faubion. Vol. 2 of *Essential Works of Foucault, 1954-1984* , ed. Paul Rabinow, 269-278. New York: The New Press.

________. 1998b. On the ways of writing history. In *Aesthetics, method, and epistemology*, ed. James D. Faubion. Vol. 2 of *Essential Works of Foucault, 1954-1984,* ed. Paul Rabinow, 279-296. New York: The New Press.

________. 1998c. What is an author? In *Aesthetics, method, and epistemology*, ed. James D. Faubion. Vol. 2 of *Essential Works of Foucault, 1954-1984,* ed. Paul Rabinow, 205-222. New York: The New Press.

Francis, Douglas R., Richard Jones, and Donald B. Smith. 2008. Destinies: Canadian history since confederation. Toronto: Nelson Education.

Fraser, Graham. Fall 2000. 'No more than ghosts make': The hauntology and gothic minimalism of Beckett's late work. *Modern Fiction Studies* 46, no. 3: 772-785.

Frelich, Lee E. and Peter B. Reich. 1995. Spatial patterns and succession in a Minnesota southern-boreal forest. *Ecological Monographs* 65, no. 3: 325-346.

Friederichs, K. January 1958. A definition of ecology and some thoughts about basic concepts. *Ecology* 39, no. 1: 154-159.

Fuller, W. A. 1975. *Tragedy in our national parks?* National and Provincial Parks Association of Canada.

Gaiman, Neil. 2001. *American Gods*. New York: HarperCollins.

________. 2005. *The Anansi Boys*. New York: HarperTorch.

Gibson, David J., Eric D. Adams, Joseph S. Ely, Danny J. Gustafson, Douglas McEwen, Tracy R. Evans. July-September 2000. Eighteen years of herbaceous layer recovery of a recreation area in a mesic forest. *Journal of the Torrey Botanical Society* 127, no. 3: 230-239.

Gilchrist, Grant and Mark L. Mallory. 2007. Comparing expert-based science with local ecological knowledge: What are we afraid of? *Ecology and Society* 12, no. 1. http://www.ecologyandsociety.org/vol12/iss1/resp1/ (Accessed March 3, 2008).

Gleason, H. A. January 1926. The individualistic concept of the plant association. *Bulletin of the Torrey Botanical Club* 53, no. 1: 7-26.

Gleason, Henry Allan. October 1917. The structure and development of the plant association. *Bulletin of the Torrey Botanical Club* 44, no. 10: 463-481.

Glitzenstein, Jeff S. and William J. Streng Donna R. Platt. 1995. Effects of fire regime and habitat on tree dynamics in North Florida longleaf pine savannas. *Ecological Monographs* 65, no. 4: 441-476.

Glover, James M. April 2000. Soul of the wilderness: Can we stop trying to control nature? *International Journal of Wilderness* 6, no. 1: 4-8.

Golley, Frank B. April 1960. Energy dynamics of a food chain of an old-field community. *Ecological Monographs* 30, no. 2: 187-206.

Golley, F. B. 1993. *A History of the ecosystem concept in ecology: More than the sum of the parts*. London: Yale University Press.

Goodman, Daniel. September 1975. The theory of diversity-stability relationships in ecology. *The Quarterly Review of Biology* 50, no. 3: 237-266.

Gordon Writing Group, Ottawa. 1997. Ottawa: Minster of Public Works and Government Services Canada. http://dsp-psd.pwgsc.gc.ca/Collection/R64-184-1997E.pdf. Accessed March 14, 2008.

Greist, David A. 1976. The carrying capacity of public wildland recreation areas: Evaluation of alternative measures. *Journal of Leisure Research* 6, no. 2: 123-128.

Grove, J. W. 1989. *In defence of science: Science, technology, and politics in modern society*. Toronto: University of Toronto Press.

Guilderson, Hugh. February 1993. Review of *Signs of the times: Deconstruction and the fall of Paul de Man,* by David Lehman. *Theory and Society* 22, no. 1: 142-147.

Gunderson, Kari and Leo H. McAvoy. April 2003. An evaluation of the wilderness and land ethic curriculum and teacher workshops. *International Journal of Wilderness* 9, no. 1: 35, 38-40.

Gyan, Prakash. 1999. *Another reason: Science and the imagination of modern India*. Princeton: Princeton University Press.

Hadley, E. B. and L. C. Bliss. Autumn 1964. Energy relationships of alpine plants of Mt. Washington, New Hampshire. *Ecological Monographs* 34, no. 4: 331-357.

Hagen, Joel B. 1992. *An entangled bank: The origins of ecosystem ecology*. New Brunswick, New Jersey: Rutgers University Press.

Hammitt, William E. 1983. Toward an ecological approach to perceived crowding in outdoor recreation. *Leisure Sciences* 5, no. 4: 309-320.

Hammitt, W. E. and D. N. Cole. 1987. *Wildland recreation: Ecology and management*. New York: John Wiley & Sons.

Hargrove, Eugene C. 1989. *Foundations of environmental ethics*. Englewood Cliffs New Jersey: Prentice Hall.

Harkin, J. B. 1957. *The history and meaning of the national parks of Canada: Extracts from the papers of the late Jas. B. Harkin, first commissioner of the national parks of Canada*. Saskatoon, Saskatchewan: H. R. Larson Publishing.

Harland, Richard. 1988. *Superstructuralism: The philosophy of structuralism and post-structuralism*. London: Routledge.

Harry, Joseph, Richard Gale, and John Hendee. 1969. Conservation: An upper-middle class social movement. *Journal of Leisure Research* 1, no. 3: 246-254.

Harshaw, H. W. and D. B. Tindall. 2005. Social structure, identities, and values: A network approach to understanding people's relationships to forests. *Journal of Leisure Research* 37, no. 4: 426-449.

Hart, John. 1998. *Walking softly in the wilderness: The Sierra Club guide to backpacking*. San Francisco: Sierra Club Books.

Hastings, Alan, Carole L. Hom, Stephen Ellner, Peter Turchin, and H. Charles J. Godfray. 1993. Chaos in ecology: Is mother nature a strange attractor? *Annual Review of Ecology and Systematics* 24 : 1-33.

Hays, Samuel P. January 1996. Comment: The trouble with Bill Cronon's Wilderness. *Environmental History* 1, no. 1: 29-32.

Hayward, C. Lynn. October 1948. Biotic communities of the Wasatch chaparral, Utah. *Ecological Monographs* 18, no. 4: 473-506.

Heatwole, Harold. Summer 1961. Analysis of the forest floor habitat with a structural classification of the litter or L layer. *Ecological Monographs* 31, no. 3: 267-283.

Hendee, John C. 1995. Universities must play a larger role in wilderness research. *Trends* 32, no. 1: 22-27.

Hendee, John C. and Chad P. Dawson. 2001. Stewardship to address the threats to wilderness resources and values. *International Journal of Wilderness* 7, no. 8: 4-9.

Hendee, John C. and Chad P. Dawson. 2004. Wilderness: Progress after forty years under the U.S. Wilderness Act. *International Journal of Wilderness* 10, no. 1: 4-7.

Herbert, Frank. 1981. *God Emperor of Dune*. New York: G. P. Putnam's Sons.

________. 1984. *Heretics of Dune*. New York: G. P. Putnam's Sons.

Hickie, Paul . January 1957. Symposium on the applications of ecology: The application of ecology to wildlife management. *Ecology* 38, no. 1: 53-56.

Hill, M. and M. Shecter. 1978. Multiple objectives in outdoor recreation planning. *Journal of Leisure Research* 10, no. 2: 126-140.

Hoffmann, C. H., H. K. Townes, H. H. Swift, and R.I. Sailer. January 1949. Field studies on the effects of airplane applications of DDT on forest invertebrates. *Ecological Monographs* 10, no. 1: 1-46.

Holling, C. S. 1992. Cross-scale morphology, geometry, and dynamics of ecosystems. *Ecological Monographs* 62, no. 4: 447-502.

Howarth, Richard B. Summer 2007. Adaptive management and the philosophy of environmental policy. *Perspectives in Biology and Medicine* 50, no. 3: 453-458.

Howell, Signe. 1996. Nature in culture or culture in nature? In *Nature and society: Anthropological perspectives*, ed. Philippe Descola and Gísli Pálsson, 127-144. New York: Routledge.

Hughes, Coley S. and Cheryl A. Estes. 2005. The influence of environmental education on environmentally responsible behaviors of undergraduate students in a traditional and non-traditional setting. *Journal of Experiential Education* 27, no. 3: 308-310.

Ingerson, Alice E. 1994. Tracking and testing the nature-culture dichotomy. In *Historical ecology: Cultural knowledge and changing landscapes*, ed. Carole L. Crumley, 44-66. Santa Fe, New Mexico: School of American Research Press.

Jackson, William B. October 1952. Populations of the wood mouse (*Peromyscus leucopus*) subjected to the applications of DDT and parathion. *Ecological Monographs* 22, no. 4: 259-281.

Jacobs, Michael J. and Catherine A. Schloeder. 1992. Managing brown bears and recreation on the Kenai peninsula, Alaska, USA. *Environmental Management* 16, no. 2: 219-254.

Jacobson, Susan K. Winter 2001. Monitoring public satisfaction in an ecosystem management framework. *Journal of Park and Recreation Administration* 19, no. 4: 83-101.

Jalkotzy, M. G., P. I. Ross, and M. D. Nasserden. 1997. *The effects of linear developments on wildlife: A review of selected scientific literature*. Calgary, Alberta: Arc Wildlife Services Ltd.

Jasen, Patricia. 1995. Chapter Five: A rest cure in a canoe. In *Wild things: Nature, culture, and tourism in Ontario, 1790-1914*, 105-132. Toronto: University of Toronto Press.

Jenkins, Keith. 2003. *Refiguring history: New thoughts on an old discipline*. London: Routledge.

Johnson, Cassandra Y. J. M. Bowker, Donald B. K. English, Dreamal Worthen. 1998. Wildland recreation in the rural South: An examination of marginality and ethnicity theory. *Journal of Leisure Research* 30, no. 1: 101-120.

Jones, Christopher D., Steve Hollenhorst, and Garth Tino. 2003. Relationships between climbing specialization, Leave No Trace Ethics and visual impacts: An empirical study in Rock Canyon Park, Utah. *Journal of Experiential Education* 25, no. 3: 348.

Joseph, Jonathan. 2001. Derrida's spectres of ideology. *Journal of Political Ideologies* 6, no. 1: 95-115.

Jubenville, Alan. 1976. *Outdoor recreation planning*. Philadelphia: W. B. Saunders Co.

Kaufman-Osborn, Timothy. 1997. *Creatures of Prometheus: Gender and the politics of technology*. Lanham, Maryland: Rowman & Littlefield Publishers.

Kavanau, J. Lee. 1966. Automatic monitoring of the activities of small mammals. In *Systems analysis in ecology*, ed, Kenneth E. F. Watt, 99-146. London: Academic Press.

Keever, Catherine. July 1950. Causes of succession on old fields of the piedmont, North Carolina. *Ecological Monographs* 20, no. 3: 229-250.

Kellner, Hans. 1998. Interview. In *Encounters: Philosophy of history after postmodernism*, Ewa Domańska, 39-66. Charlottesville: University Press of Virginia.

Kennedy, Roger. December 1996. Managing wilderness in perpetuity and in democracy. *International Journal of Wilderness* 2, no. 3: 6-9.

Killan, Gerald. 1998. Provincial parks and changing conceptions of 'protected places'. In *Changing parks: The history, future and cultural context of parks and heritage*, ed. John S. Marsh and Bruce W. Hodgins, 34-49. Toronto, Ontario: Natural Heritage.

Kim, Sang-oh and Bo Shelby. 2005. Developing Standards for Trail Conditions Using Image Capture Technology. *Leisure Sciences* 27: 279-295.

Kingsland, Sharon E. 1985. *Modeling nature: Episodes in the history of population ecology*. Chicago: University of Chicago Press.

Klint, Kimberley Ann. 1990. New directions for inquiry into self-concept and adventure experience. In *Adventure Education*, ed. John C. Miles and Simon Priest, 163-172. State College, PA: Venture Publishing.

Klugh, A. B. November 1918. A proposed classification in animal ecology. *The Journal of Ecology* 6, no. 3: 230.

Knopf, Richard C. and Richard Schreyer. 1985. The problem of bias in recreation resource decision making. In *The management of human behavior in outdoor recreation settings*, ed. Daniel Dustin, 23-37. San Diego: San Diego State University's Institute for Leisure Behavior.

Kotliar, Natasha B. and John A. Wiens. November 1990. Multiple scales of patchiness and patch structure: a hierarchical framework for the study. *Oikos* 59, no. 2: 253-260.

Kreutzwiser, Reid. 1989. Supply. In *Outdoor recreation in Canada*, ed. Geoffrey Wall, 21-41. Toronto: John Wiley & Sons.

Kromos, Cyril and Vance G. Martin. 2003. Support is building for global wilderness conservation. *International Journal of Wilderness* 9, no. 2: 4-8.

Kuss, Fred and Alan R. Graefe. 1985. Effects of recreation trampling on natural area vegetation. *Journal of Leisure Research* 17, no. 3: 165-183.

Laessle, Albert M. October 1958. The origin and successional relationship of sandhill vegetation and sand-pine scrub. *Ecological Monographs* 28, no. 4: 361-387.

Latour, Bruno. 1999. *Pandora's hope: Essays on the reality of science studies.* Cambridge: Harvard University Press.

_______. 2004. *Politics of nature: How to bring the sciences into democracy.* Translated by Catherine Porter. Cambridge: Harvard University Press.

Laven, Daniel N., Robert E. Manning, and Daniel H. Krymkowski. 2005. The relationship between visitor-based standards of quality and existing conditions in parks and outdoor recreation. *Leisure Sciences* 27, no. 2: 157-173.

Leledakis, Kanakis. 2000. Derrida, deconstruction and social theory. *European Journal of Social Theory* 3, no. 2: 175-193.

Leopold, Aldo and Sara Elizabeth Jones. January 1947. A phenological record for Sauk and Dane Counties, Wisconsin, 1935-1945. *Ecological Monographs* 17, no. 1: 81-122.

Levin, Simon A. December 1992. The problem of pattern and scale in ecology: The Robert H. MacArthur Award lecture. *Ecology* 73, no. 6: 1943-1967.

Lichter, John. 1998. Primary succession and forest development in coastal Lake Michigan sand dunes. *Ecological Monographs* 68, no. 4: 487-510.

Lime, David W. and George H. Stankey. 1979. Maintaining outdoor recreation quality. In *Land and leisure: Concepts and methods in outdoor recreation*, ed. Carlton S. van Doren, George B. Priddle, and John E. Lewis, 105-118. Chicago: Maaroufa Press.

Lindberg, Kreg and Stephen F. McCool. 1998. A critique of environmental carrying capacity as a means of managing the effects of tourism development. *Environmental Conservation* 25, no. 4: 291-292.

Lindeman, Eduard C. July 1940. Ecology: An instrument for the integration of science and philosophy. *Ecological Monographs* 10, no. 3: 367-372.

Lindeman, Raymond L. October 1942. The trophic-dynamic aspect of ecology. *Ecology* 23, no. 4: 399-417.

Lister, Nina-Marie E. and James J. Kay. 2000. Celebrating diversity: Adaptive planning and biodiversity conservation. In *Biodiversity in Canada: Ecology, ideas, and action*, ed. Stephen Bocking, 189-218. Peterborough, ON: Broadview Press.

Liu, Kam-Biu. 1990. Holocene paleoecology of the boreal forest and Great Lakes-St. Lawrence forest in Northern Ontario. *Ecological Monographs* 60, no. 2: 179-212.

Loo, Tina. 2006. *States of nature: Conserving Canada's wildlife in the twentieth century.* Vancouver: UBC Press.

Luckhurst, Roger. 2002. The contemporary London gothic and the limits of the 'spectral turn.' *Textual Practice* 16, no. 3: 527-546.

Lutz, H. J. January 1957. Symposium on the applications of ecology: Applications of ecology in forest management. *Ecology* 38, no. 1: 46-49.

Machlis, Gary E., Donald R. Field, and Fred L. Campbell. 1981. The human ecology of parks. *Leisure Sciences* 4, no. 3: 195-212.

Mackaye, Benton. July 1940. Regional planning and ecology. *Ecological Monographs* 10, no. 3: 349-352.

MacLaren, I. S. Autumn 1999. Cultured wilderness in Jasper National Park. *Journal of Canadian Studies* 34, no. 3: 3-54.

Macnab, James A. January 1958. Biotic aspection in the Coast Range Mountains of northwestern Oregon. *Ecological Monographs* 28, no. 4: 21-54.

Malin, Shimon. 2001. *Nature loves to hide: Quantum physics and reality, a western perspective.* Oxford: Oxford University Press.

Manning, Robert E. 1999. *Studies in outdoor recreation: Search and research for satisfaction*, 2nd ed. Corvallis, OR: Oregon State University Press.

_______. 2003. What to do about crowding and solitude in parks and wilderness? A reply to Steward and Cole. *Journal of Leisure Research* 35, no. 1: 107-118.

Margalef, R. November-December 1963. On certain unifying principles in ecology. *The American Naturalist* 97, no. 897: 357-374.

Marion, Jeffrey L. and Yu-Fai Leung. 2001. Trail resource impacts and an examination of alternative assessment techniques. *Journal of Park and Recreation Administration* 19, no. 3: 17-37.

Marsh, John S. 1970. Maintaining the wilderness experience in Canada's national parks. In *Canadian parks in perspective*, ed. J. G. Nelson and R. C. Scace, 123-136. Montreal, Quebec: Harvest House.

_______. 1978. Recreation and the wilderness experience in Canada's national parks, 1968-1978. In *The Canadian national parks: Today and tomorrow. Conference II: Ten years later*, ed. J. G. Nelson, S. H. Needham, and R. C. Scace, 151-177. Banff, Alberta.

Marsh, J. and G. Wall. 1982. Introduction: Themes in the investigation of the evolution of outdoor recreation. In *Recreational land use: Perspectives on its evolution in Canada*, ed. Geoffrey Wall and John S. Marsh. Ottawa: Carleton University Press.

Martin, N. D. July 1959. An analysis of forest succession in Algonquin Park, Ontario.

Ecological Monographs 29, no. 3: 187-218.

Martin, Robert A. 2004. *Missing links: Evolutionary concepts and transitions through time*. Sudbury, Massachusetts: Jones and Bartlett Publishers.

Martin, V. and M. Inglis. 1984. *Wilderness: The way ahead.* The Park, Forres, Scotland: Findhorn Press.

Mason, Herbert L. April 1947. Evolution of certain floristic associations in Western North America. *Ecological Monographs* 17, no. 2: 201-210.

Matthews, Kathleen R. and Roland K. Knapp. April 1999. A study of high mountain lake fish stoking effects in the U.S. Sierra Nevada wilderness. *International Journal of Wilderness* 5, no. 1: 24-26.

Mattson, David. 1997. Wilderness dependent wildlife: The large and the carnivorous. *International Journal of Wilderness* 3, no. 4: 34-38.

Mattyasovsky, Eugene. 1979. Recreation area planning: Some physical and ecological requirements. In *Land and leisure: Concepts and methods in outdoor recreation*, ed. Carlton S. van Doren, George B. Priddle, and John E. Lewis, 2nd ed., 71-85. Chicago: Maaroufa Press.

Mayle, Francis E. and Les C. Cwynar. 1995. Impact of the younger Dryas cooling event upon lowland vegetation of maritime Canada. *Ecological Monographs* 65, no. 2: 129-154.

McAvoy, L. and D. Dustin. 1989. Resurrecting the frontier. *Trends* 26, no. 3: 40-42.

McAvoy, Leo. 1985. Visitor control methods. In *The management of human behavior in outdoor recreation settings*, ed. Daniel L. Dustin, 11-22. San Diego: San Diego State University's Institute for Leisure Behavior.

_______. 1987. Education for outdoor leadership. In *High-adventure outdoor pursuits: Organization and leadership*, ed. Joel F. Meier, Talmage W. Morash, and George E. Welton, 2nd ed., 459-468. Columbus, Ohio: Publishing Horizons.

_______. 1990. Rescue-free wilderness areas. In *Adventure education*, ed. John C. Miles and Simon Priest, 329-334. State College, PA: Venture Publishing.

McCarthy, Patrick E. and Michael Dower. 1979. Planning for conservation and development: An exercise in the process of decision making. In *Land and leisure: Concepts and methods in outdoor recreation*, ed. Carlton S. van Doren, George B. Priddle, and John E. Lewis, 285-299. Chicago: Maaroufa Press.

McClure, H. Elliott. January 1943. Aspection in the biotic communities of the Churchill area, Manitoba. *Ecological Monographs* 13, no. 1: 1-35.

McClaren, Milton. 1990. Planet saving: The ultimate adventure. In *Adventure education*, ed. John C. Miles and Simon Priest, 445-452. State College, PA: Venture Publishing.

McCool, Stephen F. and D. N. Cole. 2000. Communicating minimum impact behavior with trailside bulletin boards: Visitor characteristics associated with effectiveness. Paper presented at the wilderness science in a time of change conference—Volume 4: wilderness visitors, experience, and visitor management. Ogden, UT: Rocky Mountain Research Station.

McCoy, E. D. and Kristin Shrader-Frechette. 1992. Community ecology, scale, and the instability of the stability concept. *PSA: Proceedings of the Biennial Meeting of the Philosophy of Science Association* 1: 184-199.

McDonald, Dennis. 1970. Population, resources & environment of man. *Lands - Forests - Parks - Wildlife* 13, no. 2: 10-16.

McDonald, Daniel and Leo McAvoy. 1997. Native Americans and leisure: State of the research and future directions. *Journal of Leisure Research* 29, no. 2: 145-66.268.

McGivney, Annette. 1998. *Leave No Trace: A practical guide to the new wilderness ethic*. Seattle: The Mountaineers.

______. 2003. *Leave No Trace: A guide to the new wilderness etiquette,* 2nd ed. Seattle: The Mountaineers Books.

McIntosh, Robert P. September-October 1975. H. A. Gleason—'Individualistic Ecologist' 1882-1975: His contributions to ecological theory. Bulletin of the Torrey Botanical Club 102, no. 5: 253-273.

McIntyre, Norman and Joseph W. Roggenbuck. 1998. Nature/person transactions during an outdoor adventure experience: A multi-phasic analysis. *Journal of Leisure Research* 30, no. 4: 401-422.

McKillop, A. B. 1979. *A disciplined intelligence*. Montreal, Quebec: McGill-Queen's University Press.

McNeill, J. R. 2003. Observations on the nature and culture of environmental history. *History and Theory Studies in the Philosophy of History: Environment and History* 42, no. 4: 5-43.

Menhinick, Edward F. Summer 1967. Structure, stability, and energy in plants and arthropods in a *Sericea Lespedeza* stand. *Ecological Monographs* 37, no. 3: 255-272.

Mentzer, Loren W. July 1951. Studies on plant succession in true prairie. *Ecological Monographs* 21, no. 3: 255-267.

Merriam Jr., L. C., K. D. Wald, and C. E. Ramsey. 1972. Public and professional definitions of the state park: A Minnesota case. *Journal of Leisure Research* 4, no. 4: 259-274.

Meyer, Adelphia Martha. July 1937. An ecological study of cedar glade invertebrates near Nashville, Tennessee. *Ecological Monographs* 7, no. 3: 403-443.

Miles, John C. 1985. The challenge of wilderness management. In *Proceedings of the 1984 conference on outdoor recreation: A landmark conference in the outdoor recreation field,* 95-102. Pocatello, Idaho: Idaho State University Press.

_______. 1990. Wilderness. In *Adventure education*, ed. John C. Miles and Simon Priest, 325-328. State College, PA: Venture Publishing.

Miller, Kenton R. 1999. International wilderness provides ecological services for sustainable living. *International Journal of Wilderness* 5, no. 3: 35-39.

Minshall, G. Wayne. December 1988. Stream ecosystem theory: A global perspective. *Journal of the North American Benthological Society* 7, no. 4: 263-288.

Miranda, Wilma and Rita Yerkes. 1987. Women's outdoor adventure programming. In *High-adventure outdoor pursuits: Organization and leadership*, ed. Joel F. Meier, Talmage W. Morash, and George E. Welton, 2nd ed., 259-267. Columbus, OH: Publishing Horizons.

Mitman, Gregg. 1992. *The state of nature: Ecology, community, and American social thought, 1900-1950.* Chicago: University of Chicago Press.

Mohr, Carl O. July 1943. Cattle droppings as ecological units. *Ecological Monographs* 13, no. 3: 275-298.

Monz, Christopher A. 2002. The response of two arctic tundra pant communities to human trampling disturbance. *Journal of Environmental Management* 64: 207-217.

More, Thomas A. 2002a. The marginal user as the justification for public recreation: A rejoinder to Crompton, Driver and Dustin. *Journal of Leisure Research* 34, no. 1: 103-118.

_______. 2002b. 'The parks are being loved to death' and other frauds and deceits in recreation management. *Journal of Leisure Research* 34, no. 1: 52-78.

Morrison, K. 1982. The evolution of the Ontario provincial park system. In *Recreational land use: Perspectives on its evolution in Canada*, ed. Geoffrey Wall and John S. Marsh, 102-121. Ottawa: Carleton University Press.

Morton, Pete. 2000. Wildland economics: Theory and practice. In *Proceedings: Wilderness science in a time of change.* Ogden, UT: US Department of

Agriculture, Forest Service, Rocky Mountain Research Station.

Mosquin, Ted. 2000. Studies and trends in Canadian biodiversity. In *Biodiversity in Canada: Ecology, ideas, and action*, ed. Stephen Bocking, 59-79. Peterborough, ON: Broadview Press.

Moss, C. E. December 1913. Evolutionary aspects of plant ecology. *The Journal of Ecology* 1, no. 4: 292-293.

Murray, Michael P. 1997. High elevation meadows and grazing: Common effects and future improvements. *International Journal of Wilderness* 3, no. 4: 24-27.

Myerson, George. 2001. *Ecology and the end of postmodernity*. Postmodern Encounters, ed. Richard Appignanesi. Duxford, Cambridge: Icon Books.

Nash, Linda. January 2005. Agency of nature or the nature of agency? *Environmental History*. http://www.findarticles.com/p/articles/mi_qa3854/is_200501/ai_n9521638. Accessed March 14, 2008.

National Outdoor Leadership School. 1995. *Leave No Trace: Outdoor skills and ethics, North American edition*. Lander, Wyoming: NOLS.

Neel, Jasper. 1988. *Plato, Derrida, and Writing*. University of Illinois Press.

Nelson, J. G. 1970. Introduction. In *Canadian parks in perspective*, ed. J. G. Nelson and R. C. Scace, 9-15. Montreal, Quebec: Harvest House.

_______. 1978. Canada's wildlands. In *The Canadian national parks: Today and tomorrow. Conference II: Ten years later*, ed. J. G. Nelson, S. H. Needham, and R. C. Scace, 707-723. Banff, Alberta.

_______. 1982. Canada's National Parks: Past, present, and future. In *Recreational land use: Perspectives on its evolution in Canada*, ed. Wall, Geoffrey; Marsh, John S., 41-61. Ottawa: Carleton University Press.

Newman, Peter, Robert Manning, Jim Bacon, Alan Graefe, Gerard Kyle. 2003. An evaluation of Appalachian Trail hikers' knowledge of minimum impact skills and practices. *International Journal of Wilderness* 9, no. 2: 34-38.

Newman, Peter, Robert Manning, Donald Dennis, Ward McKonly. 2005. Informing Carrying Capacity Decision Making in Yosemite National Park, USA Using Stated Choice Modeling. *Journal of Park and Recreation Administration* 23, no. 1: 75-89.

Nichols, George E. January 1923a. A working basis for the ecological classification of plant communities. *Ecology* 4, no. 1: 11-23.

_______. April 1923b. A working basis for the ecological classification of plant communities, part II. *Ecology* 4, no. 2: 154-179.

Niering, William A. Spring 1963. Terrestrial ecology of Kapingamarangi atoll, Caroline Islands. *Ecological Monographs* 33, no. 2: 131-160.

Nietzsche, Friedrich 1987 [1885]. *Thus Spoke Zarathustra: A book for everyone and no one.* Translated by R. J. Hollingdale. London: Penguin Books.

_______. *The Will to Power.* Translated by Walter Kaufman and R. J. Hollingdale. Edited by R. J. Hollingdale. New York: Vintage Books.

Norris, Christopher. 1998. *Deconstruction: Theory and practice.* Revised edition. London: Routledge.

Noss, Reed F. 1996. Soul of the wilderness: Biodiversity, ecological integrity, and wilderness. *International Journal of Wilderness* 2, no. 2: 5-8.

Nyland, Edo. 1970. Why environmental education? *Lands - Forests - Parks - Wildlife* 13, no. 2: 40-44.

Odum, Eugene P. 1953. *Fundamentals of ecology.* Philadelphia: W. B. Saunders Company.

_______. 1959. *Fundamentals of ecology,* 2nd ed. Philadelphia: W. B. Saunders Company.

_______. July 1964. The new ecology. *BioScience* 14, no. 7: 14-16.

_______. 1971. *Fundamentals of ecology,* 3rd ed. Philadelphia: W. B. Saunders Company.

_______. 1983. *Basic ecology.* Philadelphia: Saunders College Publishing.

_______. 1993. *Ecology and our endangered life-support systems,* 2nd ed. Sunderland, Massachusetts: Sinauer Associates.

Odum, Howard T. January 1957. Trophic structure and productivity of Silver Springs, Florida. *Ecological Monographs* 27, no. 1: 55-112.

Odum, Howard T. and Eugene P. Odum. July 1955. Trophic structure and productivity of a windward coral reef community on Eniwetok Atoll. *Ecological Monographs* 25, no. 3: 291-320.

O'Neill, Robert V. December 2001. Is it time to bury the ecosystem concept? (with full military honors, of course!). *Ecology* 82, no. 12: 3275-3284.

Page, Bob. 2000. Banff National Park: The historic legacy for biodiversity. In *Biodiversity in Canada: Ecology, ideas, and action*, ed. Stephen Bocking, 31-56.

Peterborough, ON: Broadview Press.

Park, Thomas. October 1946. Some observations on the history and scope of population ecology. *Ecological Monographs* 16, no. 4: 314-320.

Patterson, Trista, Valentina Niccolucci, and Nadia Marchettini. 2008. Adaptive environmental management of tourism in the province of Siena, Italy using the ecological footprint. *Journal of Environmental Management* 86: 407-418.

Paulson, Susan and Lisa L. Gezon. 2005. *Political ecology across spaces, scales, and social groups*. New Brunswick: Rutgers University Press.

Pearsall, W. H. March 1918. On the classification of aquatic plant communities. *The Journal of Ecology* 6, no. 1: 75-84.

Peet, Richard and Michael Watts. 2004. *Liberation Ecologies: Environment, development, social movements*, 2nd ed. London: Routledge.

Pickering, Catherine M. and Ralf C. Buckley. August 2003. Swarming to the summit: Managing tourists at Mt Kosciuszko, Australia. *Mountain Research and Development* 23, no. 3: 230-233.

Pickett, S. T. A. and John N. Thompson. 1978. Patch dynamics and the design of nature reserves. *Biological Conservation* 13: 27-37.

Pimentel, David. 1966. Complexity of ecological systems and problems in their study and management. In *Systems analysis in ecology*, ed. Kenneth E. F. Watt, 15-35. London: Academic Press.

Plant succession in the Mount Robson region, British Columbia. December 1916. *The Journal of Ecology* 4, no. 3/4: 196-198.

Porter, Warren P. and David M. Gates. Summer 1969. Thermodynamic equilibria of animals with environment. *Ecological Monographs* 39, no. 3: 227-244.

Priddle, G. B. 1978. The parks of Ontario. In *The Canadian national parks: Today and tomorrow. Conference II: Ten years later*, ed. J. G. Nelson, S. H. Needham, and R. C. Scace, 203-221. Banff, Alberta.

Priest, S. and M. A. Gass 1997. *Effective leadership in adventure programming*. University of New Hampshire: Human Kinetics.

Priestley, J. H. June 1913. The quadrat as a method for the field excursion. *The Journal of Ecology* 1, no. 2: 89-94.

Prigogine, Ilya. 1996. *The end of certainty: Time, chaos, and the new laws of nature*, collaborator Issabelle Stengers. New York: The Free Press.

Prinzipienfragen des vegetationsforschung. Ein beitrag zur begriffsklarung und methodik der biocoenologie. May 1919. *The Journal of Ecology* 7, no. 1/2: 98-100.

Rabinow, Paul. 1997. Introduction: The history of systems of thought. In *Ethics: Subjectivity and truth*, ed. Paul Rabinow. Vol. 1 of *Essential Works of Foucault, 1954-1984*, ed. Paul Rabinow, xi-xlii. New York: The New Press.

Reed, Tom. April 1999. NOLS: Wilderness education today for leadership tomorrow. *International Journal of Wilderness* 5, no. 1: 15-19.

Reid, R. A. 1978. The role of national parks in nature preservation. In *The Canadian national parks: Today and tomorrow. Conference II: Ten years later*, ed. J. G. Nelson, S. H. Needham, and R. C. Scace, 105-113. Banff, Alberta.

Ricklefs, Robert E. 1979. *Ecology*, 2nd ed. New York: Chiron Press.

Roggenbuck, Joseph W. August 2000. Visitors: Meanings of wilderness experiences in the 21st century. *International Journal of Wilderness* 6, no. 2: 14-17.

Roggenbuck, J. W., D. R. Williams, and A. E. Watson. 1993. Defining acceptable conditions in wilderness. *Environmental Management* 17, no. 2: 187-197.

Rowan, William. March 1914. Some observations on a tern colony. *The Journal of Ecology* 2, no. 1: 18-20.

Rowe, J. S. April 1961. The level-of-integration concept and ecology. *Ecology* 42, no. 2: 420-427.

Rubel, Eduard A. March 1914a. Forests of the western Caucasus. *The Journal of Ecology* 2, no. 1: 39-42.

______. December 1914b. Heath, steppe, macchia and garigue. *The Journal of Ecology* 2, no. 4: 232-237.

Russell, Edmund. January 2005. Science and environmental history. *Environmental History* 10, no. 1.

Ryan, Sean. 2005. 'Go west young man': Using navigational technologies to find oneself. *Leisure/Loisir* 29, no. 1: 95-120.

Sachroa, L. L., W. L. Stronga, and C. C. Gates. 2005. Prescribed burning effects on summer elk forage availability in the subalpine zone, Banff National Park, Canada. *Journal of Environmental Management* 77: 183-93.

Sacklin, John A. July-September 1998. Review of *Wilderness by design: Landscape architecture and the national park service* by Ethan Carr. *Leisure Sciences* 20, no. 3: 245-246.

Savage, Joseph C. 1966. Telemetry and automatic data acquisition systems. In *Systems analysis in ecology*, ed. Kenneth E. F. Watt, 69-98. London: Academic Press.

Schaffer, William M. February 1985. Order and chaos in ecological systems. *Ecology* 66, no. 1: 93-106.

Schatzki, Theodore R. 2003. Nature and technology in history. *History and Theory Studies in the Philosophy of History: Environment and History* 42, no. 4: 82-93.

Selleck, G. W., R. T. Coupland, and C. Frankton. Winter 1962. Leafy spurge in Saskatchewan. *Ecological Monographs* 32, no. 1: 1-29.

Serletis, Apostolos, Asghar Shahmoradi, and Demitre Serletis. 2007. Effect of noise on estimation of Lyapunov exponenets from a time series. *Chaos, Solutions and Fractals* 32: 883-887.

Shafer, Elwood L., John F. Hamilton Jr., and Elizabeth A. Schmidt. 1969. Natural landscape preference: A predictive model. *Journal of Leisure Research* 1, no. 1: 1-19.

Shafer, Scott C. and William E. Hammitt. 1995. Congruency among experience dimensions, condition indicators, and coping behaviors in wilderness. *Leisure Sciences* 17: 263-279.

Shantz, H. L. July 1940. The relations of plant ecology to human welfare. *Ecological Monographs* 10, no. 3: 311-342.

Shatz, Adam, 2004. The interpreters of maladies Maxime Rodinson and Jacques Derrida. http://www.thenation.com/doc/20041213/shatz. Accessed March 3, 2008.

Shelby, Bo and Bruce Shindler. 1992. Interest group standards for ecological impacts at wilderness campsites. *Leisure Sciences* 14, no. 1: 17-27.

Shelby, Bo, Jerry J. Vaske, and Rick Harris. 1988. User standards for ecological impacts at wilderness campsites. *Journal of Leisure Research* 20, no. 3: 245-256.

Shelford, V. E. October 1934. Faith in the results of controlled laboratory experiments as applied in nature. *Ecological Monographs* 4, no. 4: 491-498.

_______. July 1935. Discussion: Some marine biotic communities. *Ecological Monographs* 5, no. 3: 325-332.

Shepard, Paul. 1995. Virtually hunting reality in the forests of simulacra. In *Reinventing nature?: Responses to postmodern deconstruction*, ed. Michael E. Soulé and Gary Lease, 17-29. Washington, DC: Island Press.

Shirer, Curt . 1985. Beyond outdoor recreation. In *Proceedings of the 1984 conference on outdoor recreation: A landmark conference in the outdoor recreation field*, 87-92.

Pocatello, Idaho: Idaho State University Press.

Shogan, Debora. 1990. The paradox of physical activity in the wilderness. *Loisir et Société* 13, no. 2: 509-514.

_______. 2002. Characterizing constraints of leisure: a Foucaultian analysis of leisure constraints. *Leisure Studies* 21: 27-38.

Simmons, Greg A. 1985. The role of academic departments in outdoor recreation programs. In *Proceedings of the 1984 conference on outdoor recreation: A landmark conference in the outdoor recreation field,* 63-69. Pocatello, Idaho: Idaho State University Press.

Slobodkin, L. Basil. January 1954. Population dynamics in Daphnia obtusa Kurz. *Ecological Monographs* 24, no. 1: 69-88.

Solomon, M. E. May 1949. The natural control of animal populations. *Journal of Animal Ecology* 18, no. 1: 1-35.

Soulé, Michael E. and Gary Lease. 1995. *Reinventing nature?: Responses to postmodern deconstruction.* Washington, DC: Island Press.

Stark, Ronald W. 1966. The organizational and analytical procedures required by a large ecological systems study. In *Systems analysis in ecology*, ed. Kenneth E. F. Watt, 37-68. London: Academic Press.

Stebbins, G. Ledyard and Jack Major. Winter 1965. Endemism and speciation in the California flora. *Ecological Monographs* 35, no. 1: 1-35.

Strathcona County Parks and Recreation. 1987. Strathcona County Parks and Recreation outdoor master plan: Draft for discussion purposes only.

Swinnerton, Guy S. 1995. Conservation through partnership: Landscape management within national parks in England and Wales. *Journal of Park and Recreation Administration* 13, no. 4: 47-60.

_______. 1999. Recreation and conservation: Issues and prospects. In *Leisure Studies: Prospects for the twenty-first century*, ed. Edgar L. Jackson and Thomas L. Burton, 199-231. State College, Pennsylvania: Venture Publishing.

T[ansley], A. G. March 1914. Drude's text-book of plant ecology: Review of *Die ökologie der pflanzen* by C. Drude. *The Journal of Ecology* 2, no. 1: 51-53.

________. December 1916. The development of vegetation: Review of *Plant succession, an analysis of the development of vegetation* by F. E. Clements. *The Journal of Ecology* 4, no. 3/4: 198-204.

________. November 1922. Review of *Vocabulaire de sociologie végétale* by J. Braun-

Blanquet and J. Pavillard. *The Journal of Ecology* 10, no. 2: 245-248.

_______. February 1928. Review of *Animal ecology* by Charles Elton. *The Journal of Ecology* 16, no. 1: 163-169.

Tansley, A. G. July 1935. The use and abuse of vegetational concepts and terms. *Ecology* 16: 284-307.

_______. August 1939. British ecology during the past quarter-century: The plant community and the ecosystem. *The Journal of Ecology* 27, no. 2: 513-530.

Tarrant, Michael A. and Gary T. Green. 1999. Outdoor recreation and the predictive validity of environmental attitudes. *Leisure Sciences* 21: 17-30.

Tavin, Kevin . Winter 2005. Hauntological shifts: Fear and loathing of popular (visual) culture. *Studies in Art Education* 46, no. 2: 101-117.

Taylor, Walter P. September 1935. Significance of the biotic community in ecology. *The Quarterly Review of Biology* 10, no. 3: 291-307.

Teal, John M. July 1957. Community metabolism in a temperate cold spring. *Ecological Monographs* 27, no. 3: 283-302.

Thapa, Brijesh and Alan R. Graefe. Spring 2003. Forest recreationists and environmentalism. *Journal of Park and Recreation Administration* 21, no. 1: 75-103.

The interpretation and application of certain terms and concepts in the ecological classification of plant communities. May 1919. *The Journal of Ecology* 7, no. 1/2: 101-102.

Thornthwaite, C. W. July 1940. The relation of geography to human ecology. *Ecological Monographs* 10, no. 3: 343-348.

Thornthwaite, C. Warren. October 1931. The climates of North America: According to a new classification. *Geographical Review* 21, no. 4: 633-655.

Thruschwell, Adam. 2004. Specters of Nietzsche: Potential futures for the concept of the political in Agamben and Derrida. Working Paper Series. http://ssrn.com/abstract=969055 Accessed August 1, 2008.

Thurston, Eden and Richard J. Reader. 2001. Impacts of experimentally applied mountain biking and hiking on vegetation and soil of a deciduous forest. *Environmental Management* 27, no. 3: 397-409.

Toulmin, Stephen. 1960. *The philosophy of science: An introduction*. New York: Harper Torchbooks.

Turchin, Peter. October 1993. Chaos and stability in rodent population dynamics: Evidence from non-linear time-series analysis. *Oikos* 68, no. 1: 167-172.

Turner, James Morton. July 2002. From woodcraft to 'Leave No Trace': Wilderness, consumerism, and environmentalism in Twentieth-Century America. *Environmental History* 7, no. 3: 462-484.

United States Department of Agriculture Forest Service. 1994. *Leave No Trace!: A program to teach skills for protecting the wilderness environment*. USDA Forest Service.

Urquhart, Ian. 1998. Introduction. In *Assault on the Rockies: Environmental controversies in Alberta*, ed. Ian Urquhart, 1-5. Edmonton, Alberta: Rowan Books.

van Doren, Carlton S., George B. Priddle, and John E. Lewis. 1979. *Land and leisure: Concepts and methods in outdoor recreation*. Chicago: Maaroufa Press.

Vanderlinden, Loren and John Eyles. 2000. Public perspectives on biodiversity: Models and a case study. In *Biodiversity in Canada: Ecology, ideas, and action*, ed. Stephen Bocking, 237-270. Peterborough, ON: Broadview Press.

Vegetation of British Columbia; First and second reports of the botanical office of the province of British Columbia. September 1915. *The Journal of Ecology* 3, no. 3: 192.

Voase, Richard. Spring 2005. 'Consuming' Colourful Characters: A Foucauldian perspective on the photographed subject. *Loisir et Société* 28, no. 1: 319-336.

Vorschlag zur nomenklatur der soziologischen pflanzengeographie. May 1919. *The Journal of Ecology* 7, no. 1/2: 100-101.

Waiser, W. A. 1989. *The field naturalist: John Macoun, the geological survey, and natural science*. Toronto, Ontario: University of Toronto Press.

Wall, G. and R. Wallis. 1982. Camping for fun: A brief history of camping in North America. In *Recreational land use: Perspectives on its evolution in Canada,* ed. Geoffrey Wall and John S. Marsh, 341-353. Ottawa: Carleton University Press.

Wall, Geoffrey. 1989. Environmental impacts. In *Outdoor recreation in Canada*, ed. Geoffrey Wall, 199-229. Toronto: John Wiley & Sons.

Wall, Geoffrey and John S. Marsh. 1982. *Recreational land use: Perspectives on its evolution in Canada*. Ottawa: Carleton University Press.

Wallace, David Foster. (2003). *Everything and More: A Compact History of* ∞. New York: W. W. Norton & Company.

Warren, Karen. 1990. Women's outdoor adventures. In *Adventure education*, ed. John C. Miles and Simon Priest, 411-417. State College, PA: Venture Publishing.

Watson, Alan E. December 2004. The fundamental definition of wilderness character. *International Journal of Wilderness* 10, no. 3: 4-7.

Watson, Alan E. and Daniel R. Williams. 1995. Priorities for human experience research. *Trends* 32, no. 1: 14-18.

Watt, Kenneth E. F. July 1955. Studies on population productivity: I. three approaches to the optimum yield problem in populations of *Tribolium confusum*. *Ecological Monographs* 25, no. 3: 269-290.

_______. October 1959. Studies on population productivity II. Factors governing productivity in a population of smallmouth bass. *Ecological Monographs* 29, no. 4: 367-392.

_______. 1966a. The nature of systems analysis. In *Systems analysis in ecology*, ed. Kenneth E. F. Watt, 1-14. London: Academic Press.

_______. 1966b. *Systems analysis in ecology*. London: Academic Press.

Watts, Michael and Richard Peet. 2004. Liberating political ecology. In *Liberation Ecologies: Environment, development, social movements*, ed. Richard Peet and Michael Watts. 2nd ed., 3-47. London: Routledge.

Weaver, J. E. and T. J. Fitzpatrick. April 1934. The prairie. *Ecological Monographs* 4, no. 2: 110-295.

Wells, Philip V. Winter 1962. Vegetation in relation to geological substratum and fire in the San Luis Obispo quadrangle, California. *Ecological Monographs* 32, no. 1: 79-103.

Welton, George E. 1987. Natural freedom and wilderness survival. In *High-adventure outdoor pursuits: Organization and leadership*, ed. Joel F. Meier, Talmage W. Morash, and George E. Welton. 2nd ed., 59-63. Columbus, Ohio: Publishing Horizons.

White, Daniel R. 1998. *Postmodern ecology: Communication, evolution, and play*. Albany, NY: State University of New York Press.

White, Dave D., Troy E. Hall, and Tracy A. Farrell. 2001. Influence of ecological impacts and other campsite characteristics on wilderness visitors' campsite choices. *Journal of Park and Recreation Administration* 19, no. 2: 83-97.

Whittaker, Doug and Bo Shelby. 1988. Types of norms for recreation impacts: Extending the social norms concept. *Journal of Leisure Research* 20, no. 4: 261-273.

Whittaker, R. H. January 1953. A consideration of climax theory: The climax as a population and pattern. *Ecological Monographs* 23, no. 1: 41-78.

Wiegert, Richard G. Summer 1964. Population energetics of meadow spittlebugs (*Philaenus spumarius L.*) as affected by migration and habitat. *Ecological Monographs* 34, no. 3: 217-241.

Wilkinson, Paul F. 1992. An environmental perspective on recreation: The 'environment-recreation interaction model. *Journal of Applied Recreation Research* 17, no. 2: 178-210.

_______. Summer 2003. Ecological integrity, visitor use, and marketing of Canada's National Parks. *Journal of Park and Recreation Administration* 21, no. 2: 63-83.

Willard, Beatrice E. and John W. Marr. 1970. Effects of human activities on alpine tundra ecosystems in Rocky Mountain Park, Colorado. *Biological Conservation* 2, no. 4: 257-265.

Witt, Peter A., ed. 1993. *Journal of Leisure Research: The Wildland-Urban Interface*, Vol. 25, no. 1. Arlington, VA: National Recreation and Park Association.

Woods Gordon Management Consultants; Richard A Nuxoll Consulting Services Ltd.; MTB Consulting Ltd.. 1985. Strathcona County Recreation and Parks: Recreation and parks development plan, 1985-1989. Final report.

Woodwell, G. M. and A. L. Rebuck. Winter 1967. Effects of chronic gamma radiation on the structure and diversity of an oak-pine forest. *Ecological Monographs* 37, no. 1: 53-69.

Worster, Donald. 1993. *The wealth of nature: Environmental history and ecological imagination*. New York: Oxford University Press.

_______. 1994. *Nature's economy: A history of ecological ideas,* 2nd ed. Cambridge: Cambridge University Press.

_______. 1995a. Doing environmental history. In *Consuming Canada: Readings in environmental history*, ed. Chad Gaffield and Pam Gaffield, 16-32. Toronto: Copp Clark.

_______. 1995b. Nature and the disorder of history. In *Reinventing nature?: Responses to postmodern deconstruction*, ed. Michael E. Soulé and Gary Lease, 65-85. Washington, DC: Island Press.

Wright, Donald A. Summer 1997. W. D. Lighthall and David Ross McCord: Antimodernism and English-Canadian imperialism, 1880s-1918. *Journal of Canadian Studies* 32, no. 2: 134-153.

Wu, Jianguo and Orie L. Loucks. December 1995. From balance of nature to hierarchical patch dynamics: A paradigm shift in ecology. *The Quarterly Review of Biology* 70, no. 4: 439-466.

Wu, Jianguo and Simon A. Levin. 1994. A spatial patch dynamic modeling approach to pattern and process in an annual grassland. *Ecological Monographs* 64, no. 4: 447-464.